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MIND

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OF

PSYCHOLOGY AND PHILOSOPHY

I.—OTHER MINDS (VI.).

BY JOHN WISDOM.

Black. Let's "look down that lonely road" which leads past abandoned illusions to the security of Solipsism. Let's see what's wrong with the common or garden claims to knowledge, and by refusing in those cases to speak of knowledge set our minds on higher things, gain a glimpse of perfect knowledge, though perhaps without hope of reaching it. We shall have to reject much, indeed most, of what the common herd unthinkingly accept as real knowledge, most of what we ourselves in unthinking moments have taken to be real knowledge. To begin with, we shall have to recognise that we never really know what is still in the future, much less what would happen if this or that were to come about, or what would have happened if things had been otherwise. For when a man says "I know he'd have won if Richards had ridden him" we know that the most he really knows is that the horse turned round as the tapes went up and thereby lost some three or four lengths, that he nevertheless was beaten by no more than half a length, and that horses seldom or never turn round at the gate when Gordon Richards is riding. Does such knowledge absolutely guarantee what would have happened in this case? Certainly not. No one knows what would have happened. Maybe *this* horse would have turned round in spite of Gordon's subtlest persuasions. Maybe this horse would not have exerted himself if he had not been left behind at the start. Maybe if he hadn't got left he'd have got

bumped or jumped the path across the course, or anything of too many things. Nor is it otherwise with cars. With horses it's obvious that there are a thousand accidents and follies that may "upset your calculations", with horses it's notorious that "you never can tell", that "one never knows". What's not so obvious is that this is fatal to all knowledge. It is, however, tolerably easy to see that with cars, too, one never really knows. True, if the Bentleys are beaten by very little, and then a small but definite defect is detected in the super-chargers used by the team then we feel very sure they would have won with better super-chargers. For our experience assures us that another ·05 of a mile per hour could have been got out of them, and knowing by how little they lost it is then a matter of mathematics to decide whether they would have won or lost. But is it? Or, if you like, do we know that they would have lapped ·05 of a mile per hour faster? Even if we know that, fitted with faultless super-chargers, they would have gone faster, this doesn't guarantee that their average speed for the race would have been better—on the contrary, some other part of the mechanism might have failed to stand the slightly greater strain of the slightly increased pace. And, worse still, do we *know* that faultless super-chargers would have increased speed? We do not. We should be surprised to see a horse get away badly with Richards riding, but of course it might happen. We should be astonished if a car did no better with a superior super-charger. But of course it might not. The very change which in other cars, even in other cars of the same make, has resulted in a better performance might, in this car, have no effect, or only make it worse. Speaking of a horse, someone may say, "Then for some unknown reason it refused to move", or "Then for some unknown reason it rushed down that side turn by the White Farm. As if it hadn't been along that road a hundred times and never turned it's head". And often the tone of voice in which the speaker says "for some unknown reason" makes it plain that only his scientific upbringing prevents him saying "Then for no reason at all it . . ." It is worth noticing that instead of saying "Then for no reason . . ." or "Then for no discoverable reason" or "Then for some unknown reason . . ." the story-teller may say "Then he *took it into his head, e.g.* to rush down that side turning".[1] It's the same with cars. A

[1] The two may be used together, "For some unknown reason he took it into his head". But nevertheless there is an association between the absence of a cause (magic), the undiscoverable cause, and the psychological cause.

man may say, " My car has a mind of her own. Sometimes she'll go, sometimes she won't ". Or a man may say, exasperated by the non-uniformity of nature into speaking animistically, but obliged by the prestige of science to speak jokingly, " My car goes when she chooses ". Notice not only the ' chooses ', but the ' she ' or ' he '. If one morning you see this man struggling with his car and say pleasantly, " Won't go this morning ? What's wrong ? " he will look up with scarlet face and say, " There's nothing wrong. The damn thing just won't go ". You will shake your head at the naughty boy. So would I. We are sure that a calm and skilful mechanic would find something wrong. We are more sure of that than we are that the man who knows a lot about horses, or better, *understands* them, will find what's wrong when one of them refuses to go. He will get the horse to go, but he will quite likely not know why it stopped. But of course the mechanic may not be able to locate the trouble, that is, may not find any difference between this car and a normal car, which explains why this one won't go, and which if altered does the trick. True, when the mechanic can't find the cause of the trouble, we are sure that if the car is taken back to the makers then they will find the cause. But of course we are not *quite* sure. Maybe they'll say they can't find anything wrong, and pull the car to pieces, and perhaps make it again, or perhaps not. And if they do maybe it will go and maybe it won't. The owner of the car, who is an animistic man and believes in magic, says " I told you there was nothing wrong ". Even if we have had a modern education, and know that what he says can't really be so,[1] we are no better off than he for predicting the future. Even if we know that when one car which seems just like another behaves differently, then there is always some explanation, if only we could find it, still we are no better off than he. For our confidence in the connexion between knowledge of a car's present condition and knowledge of its future condition is greater than his only in so far as we have reduced our confidence in our knowledge of its present condition. The fact is, even with the simplest machines we get surprises. In face of these we may save the accuracy of science at the cost of its completeness, or both at the cost of confidence in the accuracy and completeness of our observations. Sometimes we watch more carefully, sometimes we say, " one of these other things must be at work here ", only at last do we say, " the ultimate laws for these

[1] Nowadays it's not done to say there was no explanation. See G. A. Paul, MIND, July, 1938, pp. 373, 374.

phenomena are statistical, *i.e.* they have free will". But whichever we do the surprises teach the same lesson—"one never knows".

And it isn't only that because of our carelessness or ignorance, or because of the overwhelming complexity of the world, we never *do* really know what would have happened if things had been otherwise, it is that we never *could* know this from information about the present and the past, or for that matter, about the present, the past and the future. And it's equally plain that we not only never do know what's going to happen in the future, but that we never could know this from information about what is now the case and about what has been the case.

Gray. But surely if we were to know (1) that a car is exactly in the condition of one which went perfectly when the starter was pressed, and (2) that if two machines or, in general, two things, are exactly alike and in exactly the same surroundings, they always react exactly alike to the same stimulus, then we would know that that car would go perfectly when the starter was pressed. Even if we haven't any such general premiss as the uniformity of nature, but only a more specific premiss that if two *cars* are exactly alike, then they always behave alike, still we should, armed with the information that this car is in the same condition and surroundings as the one which went so well yesterday, be in a position to deduce that this car will go well to-day.

Black. But what is to be meant by "*Always* in our experience when two machines are exactly alike, if one goes right the other goes right?" Does this mean merely that this always has been so? Then, as Aristotle said, no conclusion follows as to what will happen in the future in the case of the car before us. Or does it mean not merely that this always has been so, but also that it always will be so? And then, though now the premisses entail the conclusion, the argument now involves, as Mill said, a *petitio principii*. Though the premisses *appear* not to include a statement about the future, really they do.

Gray. It's only a negative or a hypothetical statement about the future.

Black. Nevertheless, the whole difficulty of our knowledge of the future now flows into the question "How do we know that if two machines are exactly alike they behave alike?" In the same way, if you say that you know that the wheels of a bicycle will go round if the pedal is turned because you know there is a connexion between the two, then the whole difficulty of the questions "How do you know the future? How do you know

the wheels will turn ? " flows into the question " How do you know there is this connexion ? Do you infer it from regular association, or do you come to see it at least in favourable cases ? Indeed, what is knowing of the presence of a *causal* connexion as opposed to knowing of the presence of a *steel* connexion ? Do you see it there, or do you guess it's there, like you guess there's a connexion between two wheels if they turn together ? In a factory you see a wonderful machine which performs a sequence of operations with machine-like regularity—first an endless band brings a jar to an opening, a valve opens, the jar is filled with Pumpkin's Pickles, an arm seals it, bang ! and the show begins again. It doesn't take you long to know what's going to happen next. Even if the sequence of operations were ever so much longer and more complicated, still, by watching long you might well come to know what was going to happen next. You might come to know this without taking a peep behind the scenes, without ever examining the machine. But of course if you could examine the machine, so much the better. Then you really would know. Then you would come on the flaw in the mechanism which makes the machine miss every hundredth jar. Our talk about causation is dominated by an unconscious phantasy [1] of this sort : Caught by a giant, you are shut in a cell. Past the window fly birds, red, green, blue, golden, and of many other colours. If you can come to be able to tell what coloured bird is coming next, you will live, but every time you make a mistake you lose a drop of blood. Soon you begin to notice a regularity in the way the birds appear together, or in succession. You may rely on statistics about this for your predictions. But if on looking carefully you can sometimes see wires connecting one bird with another, though usually you are not quite sure of this because its rather misty outside, you will use what you see of the wires to supplement your statistics, to help you to guess what bird is coming next, and to guide you as to what hypotheses about the way the birds follow one another are best worth testing by careful watching. These hypotheses you will verify, not merely by careful watching of the birds. You will vainly wipe the window pane and peer through the mist for the wires.

But causal connexions aren't wires in a mist. You say that you know there's a connexion between the turning of the pedal

[1] I here put the psychoanalytic expression 'an unconscious phantasy dictating our conscious life' instead of Wittgenstein's " We have the idea that . . . ", " We have the picture of . . . ". At the risk of being tiresome I must repeat that all this article comes from Wittgenstein. He has resolved the conflict.

of a bicycle and the turning of the back wheel, and that this is how you know what is going to happen at the other end of the bicycle as soon as you turn the pedal. It's true, of course, and plain enough, that you know that to turn this pedal will turn the back wheel, because you know they are connected—*by a chain*. But the question we are concerned with is " How do you know that to turn the pedal of a bicycle—a machine in which a pedal is connected by a chain or a cam-shaft with the back wheel—how do you know that to turn the pedal of such a machine will result in the back wheel revolving ? " It's no good answering that you know by knowing that there's a connexion between the one sort of happening and the other. For here the connexion is not something like a wire which can be observed in addition to, and judged to be responsible for, the fact that when one thing happens then another does, like the wires which were responsible for the fact that when one bird appeared another did too. The fact that there's some connexion between drunkenness and disorder just is the fact that very often when a man is disorderly in the public streets he's drunk. If the connexion is close, very close, then it just is the fact that *whenever* a man is disorderly he's drunk. But if the " whenever " does *not* include the future, then we can infer nothing about the future from the connexion (Aristotle). And if it *does*, then the major premiss couldn't be known without already knowing the conclusion (Mill).

Brown. Couldn't the connexion be a *necessary* one which experience of association enables us to *see*, like experience with marbles enables the child to come to see that 2 + 2 must always make 4.

Black. This amounts to turning the general premiss into a principle and claiming that from what is happening now one can deduce by an immediate inference without any further premisses what is going to happen.

Brown. And surely one may. From the fact that this wheel is engaged in that one, and that this one is turning I know that in a moment that one will turn. I allow that I wrongly gave the impression that knowledge of the necessary connexion was knowledge of an additional premiss. And of course it is no more an additional premiss than is " *What is foreseen must happen* " an additional premiss when I argue " I shall fall, for God foresees that I shall fall and what is foreseen must happen ".

I ought to have said plainly that there appear to be cases in which the occurrence of one sort of thing *by itself* necessitates, or *by itself* justifies an inference to the early occurrence of another. But now if there are such cases, then you are wrong in saying

that it is impossible that we should from the present know the future. And I want now to say plainly that there are such cases. Our friends let us down, our horses go lame, but " one cogged wheel can't fail to turn another ", and, as Mace says, though not in so many words, " If two ivory billiard balls approach one another, then we know what will happen when they meet ".[1]

White. Now the whole difficulty flows into the question " How do you know they are ivory ? " You don't understand the significance of your own admissions. If you admit that if the connexion is necessary, it is a principle and not a premiss, and therefore provides no further information about nature, can't you see what sort of " knowing from " this is, and what sort of " premiss-about-the-present " this is ? When to " I am putting a lever under this log ", taken as descriptive of something we really know of the present, we add " Long levers lift logs ", taken as descriptive of something we really know about the past, then we have secure premisses but they give only a problematic inference to the future.

We can make the inference demonstrative by making the major premiss more powerful, but it is not powerful enough until it includes our conclusion and shares its insecurity.

We can avoid this by making our major premiss the necessary proposition, " Long enough levers lift logs ". But this security is gained by vanishing and the demonstrativeness is retained only by making the minor premiss include our conclusion and share its insecurity.

Mace saw this, but unfortunately did not smile. Rightly he said " There are necessary connexions between the present and the future ". But no chuckle escaped him.

Black. But *are* there necessary connexions between one time and another ? I don't see that Mace and Stout [2] prove it with their examples.

White. This is a tedious business you are introducing. It doesn't matter whether there are or there aren't. If there aren't then the connexions between premisses about the present and conclusions about the future are problematic. If there are then the premisses aren't really about the present and are problematic. So that whichever is the case, conclusions about the future are problematic.

Black. I must confess I don't follow you. It does seem to me that if there were a necessary connexion between a fact or facts about the present and a conclusion about the future, then I might know the latter from the former. What I want to insist

[1] *Aristotelian Society, Suppl. Vol. XIV.* [2] *Ibid.*

upon is that Mace hasn't shown that there are necessary connexions between the present and the future. He says truly that when two billiard balls meet then they must rebound. But do we know that they will meet? It is self-contradictory to say that a brick has passed through a window without breaking it, but it is not self-contradictory to say that a brick is now hurtling towards this window but the window will not be broken.

Brown. If yesterday I fed a certain cake to a large herd of cows and to-day they are all well, then the cake *can't* have been poisoned, and if there is arsenic in this that I am giving them now they *must* be dead by the morning.

Black. Perhaps they'll disappear.

Brown. Not if they are real cows. In fairyland a hurtling brick may turn to dust just before it reaches the window, but then that proves it's a fairy brick and not an ordinary physical brick.

Black. But even if the brick were an ordinary brick, mightn't a hand come down from the clouds and clutch it in mid air? Mightn't an invisible gypsy administer, in the nick of time, an antidote to the arsenic?

Brown. Yes, but then that would be magic or the hand of God. What I know about the future when I see a brick hurtling towards a window is, if you insist on pedantic accuracy, not that the window will be broken but that it will be broken unless some hand from the clouds clutches it or it turns to dust before it reaches the window or in front of the window there's wire netting or something.

White. Or something!

> "So tell your papa where the yak can be got,
> And if he is awfully rich,
> He will buy you the creature or else he will not,
> I can not be positive which." [1]

Brown. Well, I can put it another way. If I know not merely that I am dealing with an ordinary brick, not a fairy brick, but also that I am not in fairyland, then when I see a brick hurtling towards a window I know the glass is going to break.

White. And isn't fairyland where the glass don't break, where what's fragile sometimes survives?

Black. You mean that the connexion isn't necessary between what I know about the present and what I am to learn from it of the future until what I am to know of the present includes what I am to infer about the future.

White. Exactly. You don't know that one cogged wheel will turn another unless you know that they are both of steel,

[1] Hilaire Belloc.

the best steel, *i.e.* steel that will not break—any other sense of 'best' won't do; "come from a reliable firm", "of a particular appearance", "already tested"—none of this will do. For there's no more contradiction in supposing that steel with the best pedigree, most impressive "form", and most perfect conformation should fail us than there is in supposing that a horse with these things should do so. It is nowadays logically impossible for both a standard-bred trotter and his parents to fail to trot a mile in 2 minutes 30 seconds. But this wasn't always so. This bit of logic was made quite recently in America. For "a standard-bred trotter" was defined as a horse "with a record of 2·30 or less or whose parents have a record of 2·30 or less".

Black. Even if you know that the steel is the best steel, you don't know from the present what will happen in the future, because it may cease to be the best steel by to-morrow, in half an hour's time, in a second's time. When you know it is the best steel, all you know is that if *now* the pedal were turned the wheel would turn—nothing follows about the future.

If when she hears that he has been drowned she isn't at all upset we are apt to say "She can't have loved him". And we might say "You had better not come to see her to-morrow. I am going to tell her of his death, and if, as you say, she is very much in love with him she's bound to be upset". But of course "she's bound to be" means "it's extremely probable that she will be", or at least that is all it ought to mean. This is quite plain when we consider, "You had better not come to see her when you return at the end of the year because I am going to tell her then what has happened and if, as you say, she's very much in love with him, she is *quite likely* to be considerably upset, although of course twelve months is twelve months". Here we say "quite likely" instead of 'is bound to', 'can't but', etc. Here, where between what is now the case—that she loves him—and what is predicted—her distress—is a *long* interval, we are not at all inclined to speak of a necessary connexion. Even if we are told that during that time nothing will happen to change her feelings, such as the appearance of other suitors, we feel that the mere lapse of time may well make a difference. For, as the vocalists insist, "Time on my hands with you in my arms" is one thing, but the former without the latter quite another.

We may say, "There is no explanation why Fidelity still loves him while Amaryllis already confuses him with Alfred—it's just that with Amaryllis love is very evanescent". It is clear that the fact that Fidelity was distressed to hear the news of his

death while Amaryllis was not does not necessitate that Amaryllis did not *ten years* ago love him. Now is there some number of minutes n such that if we say of a person that she loves another now then if within n minutes she hears of his death and is not distressed, it follows that we were wrong in thinking she loved him though this does not follow for $n + 1$ minutes? There is no such number of minutes. And therefore the fact that Amaryllis loves him now does not *necessitate* that she will be distressed if a *minute, a moment,* hence she hears he's been drowned. Even supposing it true that *if* a moment hence she *still* loves him then she must be distressed at the news it nevertheless does not follow from the fact that she *now* loves him that she will be distressed. For from the fact that she *now* loves him it does not follow that she will ten years, two years, or a moment hence still love him.

White. Supposing a child [1] says he knows what twice three are but when we ask him he says "Seven", and when we say "That's wrong" he says "I know it is but I know now what it is" but when we ask him he says "Eight", and next says "Eighteen", and then says "I know when you don't ask me". We don't think much of his knowing. And if he says he knows the twice time table but always makes a mistake before the end and then he says he always knows at the time he says he does and when he starts but forgets it before the end, then we give a queer sort of grin. This is so in spite of the fact that if we ask him a *week* later and he fails we don't say "He *can't* have known it when he said he did", we say, "He may have known it and forgotten it by now", or we may say, "He can't have known it *very well*". (She can't have loved him very much. It can't have been very poisonous.)

Exactly how many minutes after the child has claimed knowledge of the twice times table he must be able to get through it correctly if his claim to know it is to stand one cannot say. But it doesn't follow that there is no number of minutes such that he must be able to deliver the goods for that number of minutes if his claim is to stand. One cannot say exactly how many hairs a man must have to make good his claim that he isn't bald, but undoubtedly two are not enough. Suppose a man with only two hairs on his head says he's not bald. "Not bald", we say, "what d' you mean?". He says, "Well, you wouldn't call me bald if I had twenty thousand hairs, and if I had twenty thousand less one you wouldn't call me bald for the loss of that

[1] Another Wittgenstein example.

one hair, nor if I had twenty thousand less two, nor, etc.; and I have two hairs". Notice two things: (1) We are half inclined to allow he's right, and that a man isn't really bald unless he has no hairs on his head, that is, we are half inclined to say that being bald necessitates having no hair, while not being bald does not necessitate having more than two hairs. This inclination to accept such a notation is almost unconscious. We repress it because it would result in such very eccentric behaviour. We say "No, no", but we smile a half-condoning smile. Although he's wrong we feel he is not as wrong as one who says of a round penny that it's square.[1] We feel he's reminding us of something and when he suggests that we should speak not of baldness but of baldishness, we feel that he is suggesting a notation which brings out what is concealed by our ordinary notation which puts us in difficulties when asked whether a betwixt-and-between man is bald. And the man who says "A man isn't really bald unless he has no hairs on his head" is recommending the same sort of change in notation, though he prefers to avoid talking of every one as baldish and to speak of them as so far and not so far from being bald—while they still have a hair on their heads.

When you, Black, say "There's no necessity while there's an interval", aren't you doing the same sort of thing? The truth and value of your statement consists in the reasons you offer for it. You point out that we do offer the mere lapse of time as an excuse for a past statement ascribing a property to a thing, which property it now fails to manifest.

And you point out that we have an aversion to saying of n minutes that they provide a sufficient excuse while denying this of $n - 1$ minutes. But you then claim that we must do this or say with you that the excuse is valid however few the minutes. But you are wrong. We can do what we do do, namely count some periods as sufficient excuse, hesitate over others, and count other shorter periods as insufficient excuse. It is as absurd to say that we are wrong in doing this as it is to say that we are wrong in calling a bald man bald because he is continuously related to the unbald, or wrong in calling a sofa a sofa because it is continuously related to an armchair.

Black. It is *immensely* improbable that the best steel should lose its strength by the time I have finished speaking, but the

[1] And we are right. Or are we? For though, if ever there is a difference in kind between one thing and another, there is between a thing that is round and a thing that is square, nevertheless this embarrassing technique beginning "Is this still square?" might lead one to say "There is no difference in kind even here".

suggestion doesn't differ in kind from the suggestion that it should lose its strength in ten years time.

White. But it does differ in kind from the suggestion that it should inexplicably turn pink with blue stripes, utterly improbable as the latter suggestion is. The best steel has never turned pink with blue stripes, so it has regularly avoided this sort of thing just as much as it has regularly avoided losing its strength and turning to dust. According to you, there should be as much necessity about the not turning pink as there is about not turning to dust. But, in fact, its reputation is bound up with the latter in a way it's not with the former and that in the sense that we we shall say " It cannot *have* been the best steel " if the moment we try it it fails us. The fact of the matter is that just as there are not only cases where we argue from *This is* S to *This is* P merely because of a uniformity of association of S with P, but also cases where we do this from a necessary connexion between S and P, so there are not only cases where we argue from *This is* S to *This will be* P merely because of an association in sequence of S with P, but also cases where we do this because of a necessary connexion in sequence of S with P.

I prove this as follows: When someone says " The Kalmuks give their cows poison every night but they do well on it " we say " A queer poison ", with a queer look on our faces. This look is quite unlike the look on our faces when we say, on first seeing a diplodocus, " What an extraordinary animal ". But it *is* like the look on our faces when someone says, " I found a leopard without spots and a giraffe with a neck like a cow ".

Black. Do you or do you not want to say that there are necessary connexions between the facts of one time and the facts of another, and thus between what is at present the case and what will be the case ?

And do you or do you not want to say that we have knowledge of the future in virtue of these connexions ?

White. With great patience we draw a pretty detailed picture of the animal you and I met in the wood last night. You recognise the picture but at once you tear it up and ask, " Was what we saw a man or a horse ? " In the same way, for the detailed description of the relations between individual statements about the present and individual statements about the future you want to substitute a formula. This sort of wish is the root of philosophical difficulty. In the management of horses, even in the limited field of proper tension on the reins, there is no formula such that any ignorant and insensitive fool can be trusted to unwind from it as occasion arises, suitable treatment for the

infinite variety of situations he will meet. Likewise with sentences, even in this limited matter of necessity.

However, if you insist upon a mnemonic formula, I prefer to say that there are as necessary relations between facts separated by an interval as there are between many facts not so separated, though this necessity is, in the way we have seen, by no means independent of the interval. The longer the interval the more its adverse effect upon the necessity, but this adverse effect diminishes towards zero as the interval diminishes.

If now it is asked " Are there really any necessary connexions between any one fact and another whether they be facts of the same time or no ? And can one from one know another ? " then I want to laugh. We have looked at a great many cases of connexion and in some there is no inclination to speak of a necessary connexion, although the things connected have been associated thousands of times without exception, while in other cases there is, without any more association, an inclination to speak of a very necessary connexion. And between these extremes are intermediate cases where there appears to be an approach to, or if you like degree of, necessity.[1] When we look into (1) what it is which alters as the degree of necessity alters, and look into (2) what it is which makes the difference between a logical and a natural curiosity, we are able to describe the situation in a way which makes the question " Are there *necessary* connexions between *one* matter of fact and *another ?* " vanish.

Take the matter of natural and logical curiosities. When a man says to us, " In the wood at noon to-day I met a centaur—a creature with the body of a horse and the torso of a man ", then we are very much surprised, and say, " What an extraordinary animal ! " but we don't feel that there is in his story that sort of queerness and absurdity, that incorrectness and self-contradictoriness, which we feel there is in that of the man who says, " In the wood at noon to-day I met a horse, only it had the torso of a man ". And yet, this is the point, *they tell the same story.* We react to the second man's story with a pattern of words, namely, " What an extraordinary horse ", very like the " What an extraordinary animal " with which we reacted to the first man's story. But we bring out " What an extraordinary horse " with that queer look we have mentioned, the logical look. The look is connected with the fact that the word ' horse ' not ' animal ' appears in our second response, and because of the way the word ' horse ' is used in the second man's story. Although

[1] See Dr. Ewing, *Aristotelian Society, Suppl. Vol. XIV*, p. 68.

the two men tell the same story, they do so in different words. And the second man uses the word 'horse' in a way we feel he ought not, although we are well aware that neither he in making nor we in accepting his statement are expressing any misapprehension about what the animal he saw was like, such as might exist if he had seen it in a fog. His misapplication of the word 'horse' does not arise from ignorance of the thing to which it is applied. He is making a logical mistake.

This prepares us for what we notice when we examine the differences in a necessity series. As we imagine animals less and less like horses we find it becoming more and more absurd to call them horses. And this increasing absurdity, though it is represented as a matter of increasing improbability—the probability of their being horses or true horses—is not of that nature. On the contrary, the cases imagined are ones in which the nature of the animals is very well known indeed, in the light of noon, nothing is in doubt about them, and no betting is done on the nature of their insides or what they most like to eat. What, then, is our feeling of doubt as to whether they are horses, whether they can be horses? What is plain is that more and more of us more and more hesitate when asked "Are they horses?"

We notice that the same thing is true when we doubt whether a man whose head we can very well see is bald or not. The doubt is correlated with the degree of hesitation one has in whether to call him bald, but this correlation does not arise in the way it does when we can't see his head very well. In the latter case, besides the hesitation as to what to call him, there is a lack of confidence as to what to expect when he takes off his hat. In the case in which we can see his head very well there is no lack of confidence about how much hair there is on his head (Tautology). That is, unless his being bald is not a matter of how much hair there is on his head, there is nothing about us in favour of saying that we are doubting whether he is bald except our hesitation as to whether to call him bald. This comes out in the fact that when we can see his head well we sometimes would ask, not "Do you believe he's bald?" nor even "Is he bald?" but "Would you call him bald?" I submit that in the circumstances described the only thing that is against saying that the questions are identical is what makes one say that the second is a plainer less confusing way of asking the first.

Black. There is a very much more serious reason. "Is he bald?" is objective in a way in which "Would you call him bald?" is not. In this particular case it is most unlikely that

having said you would call him bald, you would afterwards say, influenced by argument perhaps, that now you would not call him bald. But if you did you would say that you were wrong when you said he was bald, although you would not say you were wrong in replying that you would call him bald, since at that time that is what you would have called him. And I must point out that though here there is very little difference between "Is he bald?" and "Would you call him bald?" there are other logical questions where there is the greatest difference.

White. You mean in those cases, as in arithmetic, where a man to begin with says one thing and later corrects himself, or at first does not know what to say and then gives the answer. But now the point I want to make can be put this way: In the case in which we can see a man's head well, the question "Does he now *seem* to you to be bald?" means "Do you now feel inclined to call him bald?" as opposed to the case where, when we are unable properly to see a man's head, "Does he *seem* bald to you?" means "Does his appearance at this distance lead you to expect he has hardly any hair?" The consequence is that just as "Is he bald?" means, when you can't see his head, "Would infinite investigation, including inquiry of others, leave you inclined to call him bald?" so does "Is he bald?", when you can see his head, mean "Would infinite reflexion and inquiry of others leave you inclined to call him bald?" Here (as usual) objectivity is nothing short of infinite corrigibility, infinite liability to correction from experiment. But here the experiments are *all* on ourselves with our own language.[1]

Having grasped the peculiar nature of these "baldness" *doubts* and *questions*, we can grasp the peculiar nature of the growing *absurdity* of refusing to call bald a man who is fast losing his hair, or of insisting upon calling an animal which is getting less and less like a horse, a horse.

It is soon obvious that these betwixt-and-between questions and doubts are matters of words, and then that the correlated semi-absurdities and semi-necessities are matters of words. It is then obvious that the utter absurdities and utter necessities at the ends of the series are matters of words. One says that it

[1] We all talk of infinite corrigibility in terms of something (a point) beyond the series, behind "a Veil past which we can not see, a Door to which we find no key". We all have the fatal phantasy of the prisoners in the cave which Plato brought up from the unconscious. But this phantasy, this way of talking, leads to our failure to grasp how objectivity is a matter of the infinity of the subjective. Remember the phantasy of the real unfelt and invisible weight of feathers and then of everything else. —MIND, July, 1941, p. 230.

is utterly absurd to call a certain animal a horse when one feels that, however long one reflects, one will feel disinclined to do so, and is confident that other people will be with one over this. To say that it is absurd is to vent this feeling, though it is not to say that one feels it.

We would now say that Black was right in saying that there is no difference in kind introduced by lengthening the time interval. But now we should add that this is not because there is in no cases no necessity, however short the interval, but because there is in some cases some necessity, however long the interval.

I want to try to explain in another way what I mean about the degrees and nature of necessity. The sights and sounds we see and hear appear in patterns in space and time, patterns which repeat themselves. Seeing some part of one of these patterns, one anticipates and wishes to lead one's friends to anticipate the remainder, and then one calls out a word. If to one's disappointment the remainder of the pattern fails to appear one often says one was wrong when one shouted the word. But sometimes one does not. Sometimes when one refuses to retract everyone else would also refuse, for example when to one's astonishment one finds a black swan, or a cat that likes water, or even a cow on skates. Sometimes one refuses to retract when everyone else would retract. There are again two cases: first, where everyone permanently refuses to "agree with" one; second, where everyone in the end agrees with one. In the first case, one is wrong with a wrongness which would vanish with a new language, not because what one had said would become true, but because then one couldn't make the statement one had made (Tautology). In the second case one is right in one's counting, and the opposition is proved wrong. In the first case, one remains eccentric. In the second case, one becomes the fashion. This second sort of thing may happen when an animal is discovered rather like a zebra. On reflexion (not investigation) someone says "No, it's not a zebra", but you say "Not a zebra! Not with those stripes?" "Not lovely! Not with those eyes?" This is demonstrative proof. Or is it? It isn't if it fails. But it succeeds. And therefore it is. And it's *infinite* success is the necessary connexion between having such lovely stripes and being a zebra. The same sort of thing constitutes the necessary connexion between being a zebra and preferring grass to meat. Or is this last not a necessary connexion? Wouldn't you call an animal a zebra even if it preferred meat to grass provided it had those ears and those lovely stripes? With arsenic, however lovely it looked, if it didn't kill the cattle I wouldn't call it

arsenic. By ' arsenic ' I mean a poison of a certain sort, and by a poison I mean what kills cattle, and when I say this *does* kill cattle I register my confidence that it *will* if given to them at once before it's had time to go bad or anything. So if I know this is arsenic, then I know it will kill these cattle in a few minutes if Smith now, *i.e.* this very next moment, gives it to them, and if I know I *was* right when I said it was arsenic and know that Smith is at this very moment giving it to these cattle, then I know these cattle *will* die or get ill. So from two matters of fact "*about the present*" (if Brown likes), namely that this is arsenic, and that Smith is now giving it to these cattle, I know "*another*" (if Brown likes) matter of fact, and it is one about the future, that these cattle will die. In the same way if I know these wheels are engaged in one another and of the best steel, and that the first is turning, then I know that the second will turn. And I know that the black knight's armour will turn that light sword.

Black. Unless indeed the sword is the good blade Excalibur.

White. Or the cattle come from the herds that graze the Elysian plains and are immortal.

(*To be continued.*)

II.—CERTAINTY AND EMPIRICAL STATEMENTS.

By N. Malcolm.

I.

It is a view commonly held by present-day philosophers, that it can never be known with absolute certainty that any empirical statement is true. Ordinarily they wish to include among empirical statements every statement which implies the existence of a material thing, every statement which implies that some other person is having some sensation, feeling or experience, and every statement which implies the existence of something in the past. Professor C. I. Lewis has said that " . . . all empirical knowledge is probable only ".[1] Mr. A. J. Ayer has said that " no genuine synthetic proposition . . . can be absolutely certain ".[2] And more recently, " . . . statements about material things are not conclusively verifiable ".[3] Mr. Bertrand Russell has said, " Let us take first the belief in common objects such as tables and chairs and trees. We all feel quite sure about these in ordinary life, and yet our reasons for confidence are really very inadequate."[4] And more recently, " . . . we can never be completely certain that any given proposition is true ".[5]

It is well known that Professor G. E. Moore stoutly repudiates all such " sceptical " assertions. In his " A Defence of Common Sense ",[6] Moore said that he knew *with certainty* that the earth had existed for a long time ; that there have been many other human beings upon it both before and during his lifetime ; that those other human beings have had many thoughts, experiences and feelings ; and that many human beings beside himself have known with certainty the truth of these statements. In a recent unpublished paper, Moore, in referring to some remarks made by John Wisdom, makes the following statement, " He says that he

[1] *Mind and the World-Order*, 1929, p. 309.
[2] *Language, Truth, and Logic*, 1936, p. 127.
[3] Ayer, *The Foundations of Empirical Knowledge*, 1940, p. 239.
[4] *Philosophy*, 1927, p. 3.
[5] *An Inquiry into Meaning and Truth*, 1940, p. 166.
[6] *Contemporary British Philosophy*, Second Series, edited by J. H. Muirhead.

sometimes *knows for certain* that a thing he points at is cheese; and therefore *knows in the strict sense* that he won't have to correct himself to-morrow; and goes on to say that it is often *absolutely* certain that what he points at is cheese. . . . About all this . . . I quite agree with him. It is, I should say, *absolutely* certain that this is a piece of paper with writing on it." In his "Proof of an External World",[1] Moore says that he can prove that there are material things. "How? By holding up my two hands, and saying, as I make a certain gesture with my right hand, 'Here is one hand', and adding, as I make a certain gesture with the left, 'and here is another'." [2] He goes on to say that his proof satisfies several conditions of a rigorous proof. One condition which a rigorous proof must satisfy, is that the premise must be known to be true. And "I certainly did at the moment know that which I expressed by the combination of certain gestures with saying the words 'there is one hand and here is another'. I *knew* that there was one hand in the place indicated by combining a certain gesture with my first utterance of 'here' and that there was another in the different place indicated by combining a certain gesture with my second utterance of 'here'. How absurd it would be to suggest that I did not know it, but only believed it, and that perhaps it was not the case. You might as well suggest that I do not know that I am now standing up and talking—that perhaps after all I'm not, and that it's not quite certain that I am." [3] In general, Moore would maintain that vast numbers of empirical statements are known with certainty; and he would maintain that among the empirical statements that are known with certainty are statements about material things, statements about other minds, and statements about the past.

I wish to say that what Moore says is true, and that what is said by the numerous philosophers whom he opposes is false. My main interest is, however, not in showing this, but in clarifying the nature of the dispute. I want to examine the powerful temptations that have led those philosophers to assert such a striking paradox as that no empirical statements are known with certainty. I want to consider to what extent their contention is philosophically illuminating, and to what extent it is pointless and misleading.

Let us notice first of all the non-empirical character of the dispute. When Moore holds up his hand in front of the philosopher and says that it is absolutely certain that here is a hand,

[1] *Proceedings of the British Academy*, Volume XXV, 1939.
[2] *Op. cit.*, p. 25. [3] *Ibid.*, p. 26.

and the philosopher says that it is not absolutely certain, there is no empirical evidence which Moore could produce which would make the philosopher agree with him. It would be useless for Moore to tell the philosopher to step up closer or to touch it. This shows that Moore and the philosopher do not disagree about any question of empirical fact. It may be said that what the philosopher thinks is that it is not absolutely certain that they are not dreaming or having an hallucination, while Moore thinks it *is* absolutely certain that they are not; and this, it may be said, is an empirical dispute. But the answer to this is that there is not any sort nor any amount of empirical evidence which could be submitted to the philosopher, in the face of which he would give up his contention, and agree that it is absolutely certain that they are not dreaming, and are seeing a real hand. In other words, when the philosopher says that it is not absolutely certain that Moore is showing him a hand, he is not making any empirical claim, *i.e.* any claim which could be substantiated or refuted by empirical evidence. And the arguments which he gives for his view are not empirical arguments but philosophical ones.

Let us now examine one of the most tempting of these arguments. I shall quote from Professor Lewis:—

"Obviously in the statement 'this penny is round' I assert implicitly *everything the failure of which would falsify the statement.* The implicit prediction of *all* experience which is essential to its *truth* must be contained in the original judgment. Otherwise, such experience would be irrelevant. All that further experience the failure of which would lead to the repudiation of the apprehension as illusory or mistaken is predicted in the judgment made. Now suppose we ask: How long will it be possible to verify in some manner the fact that this penny is round? What totality of experience would verify it completely beyond the possibility of necessary reconsideration? . . . it seems to be the fact that *no* verification would be absolutely complete; that all verification is partial and a matter of degree. . . . Is it not the case that the simplest statement of objective particular fact implicitly asserts something about possible experience throughout all future time; that theoretically every objective fact is capable of some (partial) verification at any later date, and that no totality of such experience is absolutely and completely sufficient to put our knowledge of such particulars beyond all possibility of turning out to be in error? So far as this is true, *all interpretation of particulars and all knowledge of objects is probable* only, however high the degree of probability. Every such judgment about the

real external world remains forever at the mercy of future possible experience. Between the immediate awareness, 'this looks round', and the objective interpretation, 'this is round', there lies all the difference between this present moment and all time; between an experience which is now complete and *had*, and a totality of possible experience which is unlimited and inexhaustible." [1]

I wish to call attention to an important feature of this line of argument. Lewis declares that no judgment about the objective character of a particular object is capable of *complete* verification. And this is because a complete verification would consist of a "totality of experience" acquired "throughout all future time". No experience acquired in a finite course of time would comprise a *complete* verification, for "the verifiable consequences of any fact last as long as time itself".[2] We might put the point in this way: No empirical statement can be completely verified because a "complete" verification would consist of an *infinite* number of verification tests. Any verification which comprises less than an infinite number of tests is only a "partial" verification, and can make the truth of the statement only probable, never certain.

We can see that if "p is completely verified" means "an infinite number of favourable verification tests have been performed with regard to p", then it certainly is the case that no proposition has ever been, or ever will be, completely verified. The reason this is so, is that it is *self-contradictory* to say that an infinite number of tests have been performed, or will be performed. It is self-contradictory to say with regard to a given proposition, p, that certain people began at a definite time, t_1, to make verification tests, and at a certain definite time, t_2, they ceased to make tests, and between t_1 and t_2 they performed an infinite number of tests. The reason, then, that Professor Lewis is sure that no empirical proposition can be completely verified, is that he *gives* such a meaning to the phrase "complete verification", that it would be self-contradictory to say that any proposition is completely verified. And he gives such a meaning to the phrase "partial verification", that it is *tautological* to say of any empirical proposition that it can only be partially verified; for this simply means that any number of tests which are performed with regard to it will be a finite number. If now the philosopher identifies "it is probable that p" with "p is partially verified", and "it is certain that p" with "p is completely

[1] *Op. cit.*, pp. 279-281. [2] *Ibid.*, p. 282.

verified ", he can then derive the philosophical proposition, " No empirical statement can be certain, but at best only probable ".

One main source of the view that empirical statements are never certain, is that some philosophers are led to interpret the meaning of the phrase " it is certain ", as applied to empirical statements, in such a way that they suppose that it is self-contradictory to say of an empirical statement that its truth is certain. If these philosophers were right, then it would be the case that when in ordinary life we say things like " It's quite certain that the ship was torpedoed ", " I know for certain that that building to the left is the Empire State ", " It's absolutely certain that he's got appendicitis ", we should be saying something self-contradictory. It is indeed often the case that when people say things of this sort, what they say is false. But nothing could be more absurd than to suppose that what they say is self-contradictory. Nothing could be more absurd than to suppose that when in ordinary life a person says " It's absolutely certain that the grain in that field is oats, not wheat ", what he means is that he or some other person or persons have performed an infinite number of tests to determine whether the grain is oats or wheat.

But I want to ignore for the moment the fact that the philosophers are mistaken in thinking that the meaning which they are led to attach to the expression " it is certain that " is the ordinary meaning, or even *an* ordinary meaning of it. I want to point out how misleading is their way of talking. They say that we cannot have certainty about empirical matters, that we can have only probability. This sounds as if our knowledge of empirical matters falls short of an *ideal*, as if we should have to get along as well as possible with the *inferior* sort of knowledge that we are actually able to attain. It turns out, however, that the philosophers have *defined* " certainty " in such a way that knowing with certainty about empirical matters is not an ideal at all, but a logically impossible state of affairs. The idea that we are unfortunate in having what is only second-best, which is often conveyed by the statement that our empirical knowledge is " only probable " or " merely probable ", is totally misleading, because the philosophers have defined " probable " in such a way that it would not make sense, would be logically absurd, to say that the truth of an empirical statement was " more than probable ". One can fall short of an ideal, only if it makes sense to speak of *attaining* the ideal. But the philosophers have defined " certainty " in such a way that it does not make sense to speak of attaining certainty. And therefore it does not make sense to speak of *failing* to attain certainty.

It is not difficult to find examples of philosophers attaching a self-contradictory meaning to an ordinary expression. Mr. Russell provided us with an example of this tendency when he said: "... there are reasons ... for being more or less distrustful of memory. It is obvious that no *direct* confirmation of a belief about a past occurrence is possible, because we cannot make the past recur. We can find confirmation of an indirect kind in the revelations of others and in contemporary records."[1] This is offered as one reason for saying that "the fact that we cannot free ourselves from dependence upon memory in building up knowledge is, *prima facie*, a reason for regarding what passes for knowledge as not quite certain".[2] Russell says that we cannot directly confirm any statement about a past event "because we cannot make the past recur". This seems to show that he is attaching such a meaning to the phrase "directly confirm", that we could directly confirm the occurrence of a past event only by witnessing now that event. But we cannot witness *now* a past event, because we could witness it now only if it occurred now; and it is self-contradictory to speak of numerically the same event occurring at two different times. In other words, Russell defines "directly confirm" in such a way that it is self-contradictory to speak of directly confirming any statement about the past. Since the fact that we cannot "directly" confirm statements about the past is supposed to be a ground for proving that "what passes for knowledge is not quite certain", it appears that Russell means by "certain knowledge" something likewise self-contradictory.

It is clear that the meaning which Russell gives to the phrase "directly confirm" is no ordinary meaning of it. Suppose that it is suspected that Robinson was in the house on the night of the murder. What would ordinarily be said to indirectly confirm this suspicion, and what to directly confirm it? If there was found in an ash-tray a butt of a cigarette of the rather uncommon brand that Robinson smoked, this might be called an indirect confirmation. But if several reliable witnesses reported that they had seen Robinson enter the house, we should say that this *directly* confirmed the suspicion. If the district attorney had only the cigarette butt for evidence, he might report that he had not yet been able to directly confirm the suspicion. He would regard direct confirmation as a goal to be striven for. In the ordinary sense of the words, "direct confirmation" is *superior* to "indirect confirmation". But Russell gives such a meaning

[1] *Philosophy*, p. 7. [2] *Ibid.*, pp. 7-8.

to the phrase " direct confirmation " that it would be ridiculous to say that direct confirmation was superior to indirect confirmation. For direct confirmation would be something self-contradictory.

Let us notice carefully the nature of Mr. Russell's procedure in this case. He takes the two expressions " direct confirmation " and " indirect confirmation ", whose ordinary meaning is such that direct confirmation of a statement about the past is superior to indirect confirmation, *i.e.* direct confirmation is an ideal state of evidence, which in many cases is difficult to obtain, but which it makes sense to speak of obtaining. Russell now gives a self-contradictory meaning to " direct confirmation ", and thereby makes the " discovery " that we can never directly confirm any statement about the past. Whereupon, it is concluded that we are forever condemned to have only evidence of an inferior sort with regard to statements about the past, and that we shall never be justified in having complete confidence in any of them. What has happened is that Mr. Russell has been tempted to invent such meanings for the phrases " directly confirm " and " indirectly confirm ", that " directly confirm " becomes self-contradictory, and " only indirectly confirm " becomes tautologous; and he has shown nothing whatever about the ordinary meanings of these expressions. Exactly the same thing happened in the case of Professor Lewis and the phrases " completely verify " and " partially verify ".

This same subtle philosophical procedure may be illustrated in connexion with a different class of empirical statements, *i.e.* statements about other minds. Philosophers are fond of saying, " You can never know for certain that another person is having an experience, or sensation, or feeling, because you can never be directly aware of another person's feelings or experiences. You can only be directly aware of your own." It then turns out that by being " directly aware " of Jones's toothache, they mean *having* numerically the same toothache that Jones is having. And so what their declaration that " you can never know for certain that another person is having a toothache " amounts to, is that " You can never have another person's toothache, you can have only your own toothache ". But this is a misleading way of saying that the expression, " I am having numerically the same toothache that someone else is having ", is self-contradictory.

What the philosphers have done, is to invent a self-contradictory meaning for the expression " knowing for certain that another person is having an experience ". They then announce regretfully, that we can never really know for certain that other

persons have experiences, and that they are not really automatons. It should be clear that the meaning which the philosophers give to that expression is an artificial one. It would be absurd to maintain that when in ordinary life a person says "It's quite certain that Joan is not pretending. I know for certain that she really is in great pain", what he means is that he is having numerically the same pain that Joan is having. And it would be absurd to maintain that his statement is in any other way self-contradictory. We use the phrase "know for certain" in ordinary life in such a way that it makes sense to say that one knows for certain statements about other minds. Of course, in many cases one does not know for certain, but has only the sort of evidence which makes such a statement probable. The philosopher proceeds to startle us with the information that we *never* know such statements for certain. But we can recover our equanimity, when we see that he has simply altered the meaning of "know for certain". We will agree with him that in his new self-contradictory sense of "know for certain", we can never know for certain statements about other minds. But we shall insist that in the old, ordinary sense of "know for certain", we can and frequently do know such statements with certainty.

II.

Mr. Ayer makes some remarks about this problem of empirical certainty which serve to throw some light upon it. He says, "Can we, in virtue of our sense-experiences, ever be sure of the truth of any proposition that implies the real existence of a material thing? . . . the answer is that if what we require to make us sure is a logical demonstration, then we cannot ever be sure . . . we do indeed verify many such propositions to an extent that makes it highly probable that they are true; but since the series of relevant tests, being infinite, can never be exhausted, this probability can never amount to logical certainty. . . .

"It must be admitted then that there is a sense in which it is true to say that we can never be sure, with regard to any proposition implying the existence of a material thing, that we are not somehow being deceived; but at the same time one may object to this statement on the ground that it is misleading. It is misleading because it suggests that the state of "being sure" is one attainment of which is conceivable, but unfortunately not within our power. But, in fact, the conception of such a state is self-contradictory. For in order to be sure, in this sense, that we were not being deceived, we should have to have

completed an infinite series of verifications; and it is an analytic proposition that one cannot run through all the members of an infinite series. . . . Accordingly, what we should say, if we wish to avoid misunderstanding, is not that we can never be certain that any of the propositions in which we express our perceptual judgments are true, but rather that the notion of certainty does not apply to propositions of this kind. It applies to the *a priori* propositions of logic and mathematics, and the fact that it does apply to them is an essential mark of distinction between them and empirical propositions." [1]

Mr. Ayer's remarks have the virtue of pointing out that, in the sense of the word " certain " of which he is thinking, it is self-contradictory to say of any empirical statement that its truth is certain; and of recognising how misleading it is, therefore, for the philosopher to say, as if it were a matter for regret, that empirical statements are never certain. But he makes a gross mistake when he goes on to say that *the notion of certainty* does not apply to empirical propositions, but instead to *a priori* propositions.

What is true is that the sense of the word " certain " in which *a priori* statements are certain does not apply to any empirical statement; for if it did, we should not call the statement an *empirical* statement. But it does not follow in the least that there is not *a* sense of the word " certain " in which it does apply to empirical statements. Suppose that a person were to reprove someone for calling an argument *circular*, saying to him that the motion of circularity does not apply to arguments, but only to material things. His remark would be entirely analogous to what Ayer says about " the notion of certainty ". It is true that the word " circular " does not have an application to arguments in the same sense in which it has an application to material things. But it does have an application to arguments. And the word " certain " does have an application to empirical statements.

Ayer has somehow been led to suppose that there is only one sense, or only one " proper " sense, of the word " certain ". And there seems to be a tendency among philosophers to suppose this. I shall try to show later that there are three senses of the expression " it is certain that " which are important in connexion with this problem. But it should be plain to anyone who reflects for a moment, that there is an extremely common usage of the phrase " it's certain that ", in which it applies to empirical statements. Hardly a one of us ever goes through a day without

[1] *The Foundations of Empirical Knowledge*, pp. 43-45.

applying that phrase to some empirical statement. It is not easy to describe the criteria which regulate this usage of the phrase, but the fact that it is not easy to do this should not lead us to say that it does not *have* a well-established usage in which it is applicable to empirical statements.

III.

I wish to discuss now an important confusion which has helped to produce the view that empirical statements can never be known with certainty. The confusion I refer to is the confusion of logical possibility with empirical possibility. Let me cite an example of it in some remarks made by Mr. Russell. In discussing whether our confidence in statements about the past is justified, he says, "Now, apart from arguments as to the proved fallibility of memory, there is one awkward consideration which the sceptic may urge. Remembering, which occurs now, cannot possibly—he may say—prove that what is remembered occurred at some other time, because the world might have sprung into being five minutes ago, exactly as it then was, full of acts of remembering which were entirely misleading . . . there is no logical impossibility in the view that the world was created five minutes ago, complete with memories and records. This may seem an improbable hypothesis, but it is not logically refutable.

"Apart from this argument, which may be thought fantastic, there are reasons of detail for being more or less distrustful of memory." [1]

It seems clear from these remarks, that Russell was inclined to regard the fact that it is logically possible that the world was created five minutes ago, as a *reason* for "being more or less distrustful of memory". And to say that it is a reason for being "distrustful of memory" can only mean that it shows that there is *some possibility* that the world was created five minutes ago. I want to say that this is absolutely false. I hold that it is an absolute confusion to say that because it is logically possible that the world was created five minutes ago, therefore there is *some* possibility that the world was created five minutes ago.

I wish to call attention to two totally different uses of the expressions "it is possible", "it might be", "perhaps it is", "it could be"; and of the corresponding negative expressions, "it is impossible", "it cannot be", "it could not be". One use of these expressions, although by no means the more frequent,

[1] *Philosophy*, p. 7.

is when they express logical possibility and logical impossibility. Now to say that it is logically possible that p is true, is to say nothing more or less than that the statement, "p", is *not* self-contradictory. And to say that it is logically impossible that p is true is to say nothing more or less than that the statement, "p", is self-contradictory. When these expressions are being used to refer to logical possibility, to say that it is possible, or that it might be, or that perhaps it is the case, that the world began to exist five minutes ago, is simply to say that the statement, "the world began to exist five minutes ago", is not self-contradictory.

But by far the more frequent use of these expressions, is a use in which they do not express logical possibility or impossibility. Let me give some examples of this other use. The doctor says, "With your leg in that condition it would be impossible for you to march in the parade to-day; but perhaps in three days you will be able to get about all right". The doctor certainly does not mean that the statement, "you will march in the parade to-day", is self-contradictory. And he certainly does not mean by his second statement, merely that it is *logically* possible that you will get about all right in three days. Or again: Something is approaching you from a distance. After straining your eyes to make it out, you say, "It could be a man on a bicycle". You certainly do not mean to be stating that "There's a man on a bicycle there" is not self-contradictory. Or again: Someone, pointing at a rushing river, may say, "None of us could possibly swim it. But we might be able to get across in our canoe". It is obvious that logical possibility and impossibility are not being talked about in this connexion.

It is quite clear that the expressions "it is possible", "it might be", etc., are used in a sense in which they do not refer to logical possibility. I shall not try to define the meaning of the expressions in this use. But it is a meaning with which we are all perfectly familiar. I shall merely give it a name, by saying that when the expressions are used in this sense they refer to "empirical possibility"; and that the corresponding negative expressions refer to "empirical impossibility".

I wish to point out an important difference in the use of these expressions, when they refer to empirical possibility, from their use when they refer to logical possibility. When one is talking about empirical possibility, it makes sense to say, "there is a slight possibility", "there is a considerable possibility", "there is some possibility", "it is barely possible", "there is hardly any possibility", "there was a possibility of it, but there isn't

any more ", " that has been possible for a long time ", " there is still a possibility ", etc. But when you are talking about logical possibility, it does not make sense to say any of these things. The point might be expressed by saying that empirical possibility admits of *degree*, and also admits of *tense*. Logical possibility admits neither of degree nor of tense. It is all right to say, " There is some possibility, although not very much, that we shall go to California " ; " There is a greater possibility that we shall go to Maine ". But if what you wanted to say was that it is *logically* possible that you will go to California, this would be a totally incorrect way to express it. For what you say when you say that it is logically possible that *p*, is that " *p* ", is not self-contradictory. And there are no *degrees* of being self-contradictory. One statement cannot be more or less self-contradictory than another. Likewise, in discussing your plans for the summer, you could say, " There is *still* the possibility that we shall go to California ". Or : " A year ago it wasn't possible for us to go to California. But now it is." But you could not correctly use that sentence to express the fact that it is logically possible that you will go to California. For it does not make sense to talk about one and the same statement having been self-contradictory at one time and not at another, or *continuing* to be self-contradictory, in the way in which a watch may continue to keep good time.

The reason it is important to make these distinctions is this : Philosophers are commonly led to confuse these two entirely different uses of the expressions we are considering. For example, they conclude from the fact that so and so is possible, where this expresses *logical* possibility, that therefore there is *some* possibillity that so and so is the case. Now the statement, " There is some possibility that *p* ", *entails* the statement, " It is not absolutely certain that not-*p* ". In this way the philosophers obtain an argument for saying that no empirical statements are absolutely certain ; for they can truly say of any empirical statement that its falsehood is possible, in the sense of logically possible.

When Mr. Russell was inclined to regard the fact that there is no logical impossibility in the view that the world sprang into being five minutes ago, as a reason for " being more or less distrustful of memory ", he presented a flagrant example of the fallacy of confusing logical and empirical possibility. He seems to have thought that he could infer from the fact that the world *might* (logically possible) have sprung into existence five minutes ago, that there is *some* possibility that it actually did. I have

tried to show that this is a completely fallacious inference. With regard to any proposition whatever, it does not *follow* from the fact that p is logically possible that there is the least possibility that not-p is false.

Let us notice another illustration of the commission of this fallacy. Mr. Ayer discusses the question of whether I can at any time be sure that I am not dreaming. He says " the answer is that if what is here meant by ' being sure ' is ' being able to give a conclusive demonstration ', then it is true that I cannot at any time be sure that I am not dreaming. I am able, in fact, to convince myself that I am not, by putting my perceptual judgments to the test of further experience and finding that they are substantiated. But since there is no theoretical limit to this process of testing, it is *always* logically possible that I am mistaken. However many favourable tests I may make, *the possibility still remains* that my subsequent experiences will consistently be such as to make me conclude that the perceptions that I had to my own satisfaction proved to be veridical were not so really, and that I was dreaming after all." [1]

Mr. Ayer has slipped from the language of logical possibility to the language of empirical possibility. It is true that the statement " I am dreaming " is not self-contradictory. *That is what it means* to say that it is logically possible that I am mistaken in thinking that I am not dreaming. It does not make sense to say that this possibility *always* exists, because it does not make sense to talk of its *ceasing* to exist. The extent to which I verify the statement " I am not dreaming " is *irrelevant* to the existence of this logical possibility. There is no connexion between verification and logical possibility or impossibility. Thus Ayer speaks most misleadingly when he says, " However many favourable tests I may make, the possibility (logical) still remains ", etc. For his remark seems to imply that the number of tests *could* have a bearing on the existence of the sort of possibility of which he is speaking. But, furthermore, he speaks incorrectly when he says that " the possibility still remains ", etc. He has lapsed into language which is appropriate only to empirical possibility.

From the statement that it is logically possible that I am dreaming now, Ayer moves to the statement that the possibility still remains that I am dreaming now. But this is an illegitimate move. For the latter statement implies that there is *some* possibility, or *a* possibility that I am dreaming now. And it does not follow from the fact that it is logically possible that I

[1] *The Foundations of Empirical Knowledge*, pp. 42-43. My italics.

am dreaming now, that there is *any* possibility that I am dreaming now. And it is, in fact, the case at this moment that there is not the least possibility that I am dreaming. To point out that the statement " I am dreaming " is not self-contradictory, is to point out something entirely irrelevant to this fact.

This source of philosophical confusion might be removed if philosophers were to substitute the language of " self-contradictory " for the language of " logically possible ". What I mean is that instead of saying " it is logically possible that p ", or " it might be that p ", or " perhaps p ", it might be better to say " p is not self-contradictory ". Because of the great similarity in the form of the expressions, there is a strong temptation to move illegitimately from " it is logically possible that p ", or " it might be (logically) that p ", to " there is some possibility that p ". And from this one infers, properly, that it is not certain that not-p. This source of error would be removed if philosophers possessed only *one* language of possibility, *i.e.* the language of empirical possibility.

If instead of talking about logical possibility and impossibility, philosophers talked about statements being self-contradictory or not self-contradictory they would not be so inclined to say that no empirical statements are *completely verifiable*. From the fact that there is *still a* possibility that not-p, it *follows* that p has not been completely verified. Philosophers are tempted to infer from the fact that it is logically possible that not-p, that there is *still a* possibility that not-p. They then generalise this argument to show that no empirical statement is completely verifiable. But if, instead of saying that it is logically possible that not-p, they simply said that not-p is not self-contradictory, perhaps they would not be so likely to infer that there is *a* or *some* possibility that not-p; and to conclude therefore that p is not completely verified.

In any case, it *should* be clear that *verification* applies only to empirical statements. Since verification applies to empirical statements, *complete* verification applies to empirical statements. It should be clear, too, that a statement is not an *empirical* statement unless it has a negative which is not self-contradictory. It *follows* from the fact that p is an empirical statement that not-p is not self-contradictory. And it *follows* from the fact that p is an empirical statement that p is capable (logically) of complete verification. Thus it is fallacious to argue that because not-p is not self-contradictory, therefore p cannot be completely verified. And this is the fallacy that the philosophers commit when they argue that no empirical statement can ever be

completely or conclusively verified, because *no matter* to *what* extent it has been verified, it is "always" possible or "the possibility still remains" that the statement is false.

IV.

A. The phrase "it is certain that" has an application to three different sorts of statements, *i.e.* to sense-statements, to *a priori* statements, and to empirical statements. To bring out the differences in these applications of the phrase, will help to explain the existence of the view that empirical statements are never known with certainty.

By "sense-statements" I mean statements of the sort, "I have a pain", "I am tasting a sweetish taste", "This looks brown to me", "This feels soft to me". They are to be opposed to statements of the sort, "I have a bad tooth", "This has a sweetish taste", "This is brown", "This is soft". When philosophers say that no empirical statements are known with certainty they are not generally referring to statements of the first sort. They generally use the phrase "empirical statement" in such a way that statements of the second sort would be included among empirical statements, while sense-statements would not be included among empirical statements. It is in this way that I am using the phrases "empirical statement" and "sense-statement".

By "sense-statements" I mean statements of the kind to which philosophers have referred by such descriptions as "statements which report what is immediately given in experience", which "report a present datum", and "cannot be refuted by subsequent experience". Empirical statements, on the other hand, they have characterised as containing "predictions as to what future experience will give", and as "going beyond the present datum".[1] It has also been said of sense-statements that they are "incorrigible". By this is meant that when you say "I have a pain", or make some other sense-statement, you cannot be mistaken about the facts. You may commit the linguistic error of expressing your meaning with the wrong words. But if you do express your meaning with the right words, and if you are not telling a lie, then it follows that your sense-statement is true. Empirical statements, on the contrary, are "corrigible".

[1] When Mr. Russell speaks of "basic propositions", he means to be referring, I am sure, to statements of the sort that I call sense-statements; but it seems to me that all of his definitions of the expression "basic proposition", are unsatisfactory. Cf. *An Inquiry into Meaning and Truth*, pp. 171-174, 187-190.

By this is meant that when you make an empirical statement you *can* be mistaken about the facts. Even if you express your meaning correctly and are not lying, it does not follow that your empirical statement is true. For you can be mistaken in thinking that what you assert to be the case is in fact the case. Sense-statements are not open to this type of error, and that is why they are called " incorrigible ".

Now many philosophers would say this sort of thing : that you can know with absolute certainty that you have the sort of pain ordinarily called toothache, but that you cannot ever be certain that you have a tooth in bad condition ; that you can know for certain that something *looks* brown to you, but you can never know for certain that it *is* brown ; that you can know with perfect certainty that something feels soft to you, but you can never know with perfect certainty that it *is* soft. That, in short, you can know sense-statements, with certainty.

Let us consider a little further what is meant by the " incorrigibility " of sense-statements. To say that sense-statements are incorrigible means that when you make a sense-statement you *cannot* be mistaken about the facts. But in what sense is " cannot " used here ? Is it the sense of " cannot " in which it is true that a man cannot swim the Atlantic ? Not at all. For although it would be false, it would *make sense* to say, " So and so swam the Atlantic yesterday ". In the true statement " A man cannot swim the Atlantic ", the " cannot " is used in a *factual* sense. And the absurdity of which one would be guilty in supposing that someone swam the Atlantic yesterday would be a *factual* absurdity.

But to say that when one makes a sense-statement such as "I have a sharp pain", one *cannot* be mistaken about the facts is to use " cannot " in a *logical* sense. For what this statement means is that *it does not make sense* to say, " I think I have a sharp pain, but perhaps I am mistaken " ; or to say, " So and so says that he has a severe headache, but perhaps he is mistaken ". The absurdity of which one would be guilty if one said these things would be a *logical* absurdity. What is meant by saying that a person *cannot* be mistaken as to the truth of a sense-statement is less ambiguously expressed by the statement, that it is *nonsensical* to say that perhaps a person is mistaken as to the truth of the sense-statement he has uttered. To say, therefore, that sense-statements are " incorrigible " is to say that it does not make sense to question them. To say this is to state the defining characteristic of sense-statements.

When the philosopher says that sense-statements can be known

with certainty, but that empirical statements cannot, what he is doing is restricting the use of the phrase " known with certainty" in such a way that a statement will be capable of being known with certainty, if and only if it is an incorrigible statement. In other words, he is proposing to use the phrase " is known with certainty " in such a way that it will be *equivalent* to the phrase " is incorrigible ". But then we see that his pronouncement that sense-statements can be known with certainty, but empirical statements cannot, is a plain truism. For it amounts to saying that sense-statements are incorrigible, and empirical statements are corrigible. And incorrigibility is the defining characteristic of sense-statements, while corrigibility is a defining characteristic of empirical statements. Of course, no empirical statement can be incorrigible, for if any statement were incorrigible we should not call it an empirical statement. The philosopher's pronouncement really amounts to saying that empirical statements are not sense-statements—which is, of course, true. If it were the case that the only proper use of the phrase " is known with certainty " was such that its meaning was equivalent to that of the phrase " is incorrigible ", then no person could say that an empirical statement was known with certainty, and say something true. For he would be saying that an empirical statement was incorrigible, which would be logically absurd.

Let us turn for a moment to the philosophers who say that *a priori* statements can be known with certainty, but not empirical statements. They may argue the point by saying that *a priori* statements *cannot* be false, while empirical statements *can* be false. But they express their meaning ambiguously; for there is a perfectly proper and ordinary sense of " cannot " in which many empirical statements cannot be false, *e.g.* " it cannot be false that the military disaster which France suffered in 1940 has brought unhappiness to many persons ". What the philosophers mean when they say that an *a priori* statement cannot be false is better expressed by saying that the negative of an *a priori* statement is self-contradictory. Their declaration that only *a priori* statements can be known with certainty is a disguised recommendation that the phrase " it is certain that " be applied to a statement if and only if the statement has a self-contradictory negative. It is a proposal to use the phrase " it is certain that *p* ", so that it will be *equivalent* to the phrase " *p* has a self-contradictory negative ". And this is the way that the philosophers are using the word " certain " when they say that no empirical statement is ever certain, but only *a priori* statements are. So what they are really saying is that no

empirical statement has a self-contradictory negative, while every *a priori* statement does have a self-contradictory negative. And this is an absolute tautology. For it is the defining characteristic of an *a priori* statement that it has a self-contradictory negative, and it is a defining characteristic of an empirical proposition that it does not have a self-contradictory negative.

B. We have seen that an important source of the philosophical view that empirical statements are never certain, lies in the fact that the philosophers compare empirical statements with *a priori* statements and with sense-statements. Certain striking differences between empirical statements and statements of the other two types makes them want to predicate certainty of the latter only. In other words, they want to use the word "certain" in such a way that a statement will be called "certain" only if it is either a sense-statement, or an *a priori* statement. They want to abolish the use of the word "certain", in which it is applied to empirical statements. They want to abolish one of the three uses of the word, and retain the other two.

What I wish to point out is that the two uses of the word "certain" which they want to retain are *degenerate* uses, and that the use they want to abolish is *non*-degenerate. Let me explain this. When it is said that a sense-statement is certain, what this means is that it does not make sense to doubt or question it. And when it is said that an *a priori* statement is certain, what this means is that the negative of it is self-contradictory. Now the defining characteristic of a sense-statement is that it does not make sense to question it. And the defining characteristic of an *a priori* statement is that it has a self-contradictory negative. Therefore to say of a sense-statement that its truth is certain is to say nothing more nor less than that it is a sense-statement; and to say of an *a priori* statement that its truth is certain, is simply to say that it is an *a priori* statement. The proposition "p is a sense-statement and p is absolutely certain", is a flat tautology. And so is the proposition "p is an *a priori* statement and p is absolutely certain". In both cases the phrase "p is absolutely certain" adds nothing whatever. To say of an *a priori* statement, or of a sense-statement, that its truth is certain is to say something trivial and non-informative. That is what I meant by saying that the uses of the word "certain", in which it is applied to statements of these two types, are degenerate.

But the use of the word "certain" in which it is applied to empirical statements is by no means degenerate. The proposition "p is an empirical statement and p is absolutely certain"

is far from being a tautology. To say of an empirical statement that it is certain is to say something highly informative. It is informative on two grounds. First, it says that the empirical stat ment is *true*, and not false. Some empirical statements are true and some false, and nothing could be more informative than to say to which class it belongs. Second, to say that the statement is certain is to say that it is not merely probable. Saying this, describes to some extent the nature of the *evidence* for the statement. It tells us something which we could not possibly know, by merely knowing that the statement is an empirical one.

Thus the philosophers who wish to abolish the application of the word " certain " to empirical statements are in a very queer position. For what they propose to do is to take away the use of the word " certain ", in which to say that a statement is certain is to say something significant and worth listening to. And they propose to keep only uses of the word, in which to say that a statement is certain is to say something absolutely trivial. If we followed out their recommendation, the word " certain " would become a worthless word.

Some philosophers, who admit that there is a perfectly proper sense of " certain ", in which empirical statements can be certain, are inclined, nevertheless, to talk as if sense-statements or *a priori* statements have a *superior* kind of certainty. I think that it is rather senseless to speak of the certainty of empirical statements as being either " inferior " or " superior " to the certainty of sense-statements or of *a priori* statements, since entirely different senses of the word " certain " are being talked about. But if I were compelled to say one or the other, I should prefer to say that empirical certainty is the superior certainty. And the reason which I should give is that to say of either a sense-statement or an *a priori* statement that its truth is certain is to say something tautological and utterly trivial. While to say of an empirical statement that its truth is certain, is to say something significant and informative.

C. Let us consider another aspect of the recommendation to discontinue the application of " certain " to empirical statements. What the philosophers want us to do is to substitute for the word " certain ", the expressions " probable " or " highly probable ". No matter how satisfactory is the evidence for p, we are always to say " it is probable that p ", never " it is certain that p ". The question I wish to raise is, supposing that we did adopt the recommended alteration in our language, should we have gained anything by it ?

It is necessary to remind ourselves of the important differences between the cases which we, in our present language, describe as cases of certainty, and the cases which we at present describe as cases of probability. Consider this example: your car begins to choke, sputter and lunge in the way it does when it is out of gas. You say, "Probably the gas tank is empty". You then get out and test the tank with a measuring stick which comes out dry. You say, "It's certainly bone dry". The philosophers would have us still say "probable" in the latter case; and even if the tank had a removable top, which we removed and saw by the broad daylight that it was empty, they would allow us to say nothing more than "It's highly probable that it's empty". Or again: A doctor observes the symptoms of pain, fever, and the way the abdomen of his patient feels. He writes in his report, "Ruptured appendix highly probable". He then operates, and sees the ruptured condition of the appendix with his eyes. Is he then to announce to the students in the amphitheatre, that it is "highly probable" that this is a case of ruptured appendix"? Or again: Your wife frowns and walks out of the room. You say, "Very likely she's displeased with me". Compare this with the case where she screams, sobs, goes home to mother, and institutes divorce proceedings. The philosophers would have you describe the latter situation, too, by saying, "It's very likely that she's displeased with me". Or again: On the basis of knowledge of past performance, you say, "It's highly probable that Joe Louis won by a knockout last night". Later you see the newspapers and talk with dozens of people who saw the fight. Are you to continue to say that it is "highly probable" that Louis won by a knockout?

The point of these examples is to emphasize the striking differences between the cases which we ordinarily call cases of probability, and the cases which we ordinarily call cases of certainty. How different is the doctor's evidence for saying that the patient has a ruptured appendix, when he has only observed the pain and fever symptoms and felt the abdomen, from his evidence for saying it when he has opened the abdomen and seen the ruptured appendix! What a difference there is in the nature of the evidence for saying that Joe Louis won last night, when in one case the evidence is Louis's many victories in the past and the mediocre past performance of his opponent, and in the other case the evidence is that 20,000 people saw the knockout blow! *The function of the distinction*, in ordinary language, between *probability* and *certainty, is just to describe such differences.* If we were to abolish this distinction, we should have abandoned an important piece of our descriptive language.

What the philosophers' recommendation amounts to, is that we should *stretch* the application of the phrase " highly probable ", so that it would apply to not only statements of the sort which we now call highly probable, but also to statements of the sort which we now call certain. But if we adopted this proposal, how then could we express the differences between what we *now* say is " highly probable but not quite certain ", and what we now say is " absolutely certain " ? If an anxious parent wanted to know whether his child had a ruptured appendix, and the doctor said that it was " highly probable " that it was ruptured, he would want to know whether it was highly probable in the *old* sense, or " highly probable " in the *new* sense. In our revised language, it would be impossible to tell him which it was. We could tell him only if we *invented a new word* which took the place of the banished word " certain ". We should have to invent a new word which would enable us to express the same old distinction between probability and certainty, that we had tried to abolish. Thus we should have gained nothing by attempting to alter ordinary language.

There are words in our language that operate in *pairs*. For example, there is " large " and " small ", " hot " and " cold ", " fast " and " slow ", " probable " and " certain ". Suppose that we banished the word " small " from the language, and applied the word " large " to everything, both large and small. It is obvious that if in describing an object to someone you said that it was " large " that would convey no information to him at all about the size of the object. The word " large " would drop out of the language, because it would be a *useless* word. It is essential to the meaning of " large ", as it is now used, that large is *contrasted* with small. If large ceases to be contrasted with small, the word " large " loses its meaning.

Similarly, if " it's probable " or " it's highly probable " were to precede *every* empirical statement, these expressions would lose their meaning. For it is essential to the meaning of " probable " and " highly probable ", that probability is *contrasted* with certainty. That is why it makes sense to say, " It's highly probable but not quite certain ". If the application of " certain " to empirical statements was abolished, the word " probable " would also cease to be applied to them. For it would have become a useless piece of language, a word which conveyed no information.

It would be fruitless to argue that probability would be contrasted with certainty, in the sense of " certain " in which sense-statements and *a priori* statements are certain. For we have

seen that to say that a sense-statement is certain, simply means that it is a sense-statement; and to say that an *a priori* statement is certain, simply means that it is an *a priori* statement. If we altered our language in the way that the philosophers want, then to say that a statement was certain would simply mean that it was not an empirical statement, but was either a sense-statement or an *a priori* statement. And to say that a statement was "probable" would simply mean that it was an *empirical* statement. But the words "probable" and "certain" would no longer be capable of being used to call attention to differences *within* the class of empirical statements. Those words could no longer indicate the differences in the evidence for various empirical statements. But it is important that we should have *some* set of words which would indicate these differences. And if that function were taken away from the words "probable" and "certain", we should have to invent a new pair of words which would perform that same function.

V.

In this concluding section, I wish to discuss the chief sources of the philosophical view that no empirical statements are known with certainty, but are at best only probable.

A. One of the main things which has led philosophers to say that the statements which we call certain are really only highly probable, has been the desire to point out that the differences in the evidence for statements that we call probable and for statements that we call certain are only differences of *degree*. Their point can best be brought out by the aid of examples. Suppose that a small child says that it saw a lion in the street. You might dismiss this as a fancy, or as the wrong use of a word. But then several other older children say they saw it too, and insist that it was a lion. You might then be inclined to say that there was some probability that they saw a lion. Then you find that a thousand adults swear that they saw a lion in the street, and that their stories and descriptions are in perfect agreement. You would say then that it is absolutely certain that there was a lion in the street. Now the philosophers wish to say that differences in the *maturity* of the observers, and differences in the *number* of observers are differences of degree only—that there is no difference of *kind* between the case where you say that the evidence is conclusive and the case where you say that the evidence is not conclusive. Or consider this example: You hear a familiar voice outside the window, and exclaim, "That is

probably Charles!" He then walks into the room, and you say, "It certainly is Charles!" Now what the philosophers want to say is this: In the case of mere probability, what you had for evidence was some of the auditory data associated with Charles. In the case of "certainty" you had the visual data associated with Charles, in addition to the auditory data. But the difference between the two cases is really only a difference in the *amount* of data—it is really only a difference of degree.

The philosophers' claim might be generalised in this way: The differences between what we call probability and what we call certainty are differences in the number of observers, or the number of observations, or the amount of data. The difference between "partial verification" and "complete verification", as we use these expressions in ordinary language, consists principally in a difference in the number of favourable tests. Likewise, the difference between "conclusive" and "inconclusive" evidence is of this sort. But differences in the number of observers or tests, or in the amount of data, are only differences of degree. No line can be drawn which will sharply separate cases of "conclusive" evidence from cases of "inconclusive" evidence, cases of "certainty" from cases of "probability". The differences are not differences of *kind*. Therefore, why can it not be said that the cases of what we call "certainty" are really cases of very high probability?

I agree, on the whole, with this way of describing the nature of the differences between the sort of evidence which makes a statement probable, and the sort of evidence which makes it certain. I think that this way of describing it may even be illuminating. If one had the idea that certainty is a mysterious quality, which inexplicably belongs to some statements and not to others, then it would be salutary to argue that the difference between certainty and probability is only one of degree. When Professor Moore is engaged in maintaining, correctly, with regard to some empirical statement, that its truth is absolutely certain, and is asked by a philosopher *how* he knows that it is absolutely certain; his frequent reply is, that he doesn't know *how* he knows, but he certainly *does* know. People, listening to Moore, sometimes get the impression that Moore thinks that it is by some sort of *intuition* that he discovers whether the truth of a statement is certain. They get the impression that Moore thinks that certainty is a simple, indefinable quality like yellow, which unaccountably attaches to some statements and not to others. If anybody ever does think this, then it would be well to give him the above line of talk, about the nature of the differences between probability and certainty.

But I want to point out how *arbitrary* it is to say that the differences between probability and certainty are not differences of *kind*. Why not say that the difference between the evidence which you have for the presence of Charles, when you only hear a familiar voice at the window, and the evidence which you have when you see him enter the room, is a difference in the *kind* of evidence? Why not say that the difference berween the testimony of one child, and the testimony of a thousand adults, is a difference in the *kind* of evidence? What lies behind this arbitrary use of the word " kind " is that the philosophers want to say that a difference in the nature of the evidence for two statements is a difference in " kind ", if and only if the " evidence " for one statement is a *demonstrative proof*, and the evidence for the other statement is *not* a demonstrative proof. To give a demonstrative proof of a statement would consist in showing that the statement is logically deducible from *a priori* premises. When the philosophers say that the difference between certainty and probability is not a difference in *kind*, what they mean is that the evidence for a statement, the truth of which is " certain ", does not consist of a demonstrative proof; any more than does the evidence for a statement, the truth of which is only probable, consist of a demonstrative proof. And of course this is true. If any statement is capable of demonstrative proof, then it is not an *empirical* statement, but an *a priori* statement. So what the philosophers are saying, when they say that the difference between certainty and probability is not a difference in kind, is that the statements which are " certain ", as well as the statements which are " probable ", have, after all, only the sort of evidence that empirical statements can have. And this is a truism. The philosophers' statement that the difference between probability and certainty is not one of kind, presents, in a disguised way, the familiar recommendation that we should restrict the application of the word " certain " to *a priori* statements.

Suppose, however, that we agreed to say that, since the differences between probability and certainty are only differences in the number of observers, number of tests, amount of data, etc., therefore the differences are of *degree* only. Would it follow that statements which we call " certain " are *really* only probable? It is easy to see that it does not follow in the least. In exactly the same sense of " difference of degree ", the difference between being bald and having a full head of hair is only a difference of degree. That is, it is a difference in the *number* of hairs on the head. To argue that, because the difference between " certain " and " probable " is only one of *degree*, therefore all empirical

statements are probable only—is exactly analogous to arguing that, because the difference between " being bald " and " having a full head of hair " is only one of degree, therefore all men are really bald. The point is, that it is the *function* of the words " certain " and " probable ", in ordinary language, to describe those differences of degree in the evidence for various statements —just as it is the function of " bald " and " full head of hair " to describe the analogous differences of degree. The philosophers show an inclination to argue that since they are " merely " differences of degree, therefore they are not really important. But the fact is that differences of degree are extremely important, as any bald-headed man will tell you.

B. The question which I wish to discuss now is suggested by some remarks made by Mr. Russell. They are as follows: " We have therefore inductive grounds for holding (on a common-sense basis) that when I ' see a cat ' there probably is a cat. We cannot go beyond ' probably ', since we know that people sometimes see cats that are not there, for instance in dreams." [1] There is no doubt that Mr. Russell intended this argument to be a perfectly general one. He meant to say that whenever a person makes any perceptual judgment, *e.g.* that he sees a house or a table, or that he hears a bell, his judgment can be no more than *probably* true, *because* people have sometimes " seen " and " heard " things of that sort in dreams, or in hallucinations.

If we take this argument at its face value, it seems to me to be an extremely bad one. It is simply absurd to argue that *no* perceptual judgment is certain, on the grounds that *sometimes* when people have made perceptual judgments they have been mistaken, or that *sometimes* people have had " perceptual experiences ", in dreams or hallucinations, which seemed veridical but were not. An analogous line of argument would be to say that it is not certain that I am now alive, because *some* people have died; or to say that it is not certain that I am not standing on my head now, because *sometimes* in the past other people and myself have stood on our heads; or to say that it is not certain that I did not have fish for breakfast, because *sometimes* people have fish for breakfast. If Russell's argument is taken at its face value, it seems to be a grotesque perversion of valid inductive argument.

But I think that Russell was trying to state a very real and serious difficulty, which he did not clearly express. He wanted to say, I believe, what many philosophers have been inclined to

[1] *An Inquiry into Meaning and Truth*, p. 151.

say—namely, that a non-veridical perceptual experience may exactly resemble a veridical perceptual experience. But if there need be no "intrinsic difference" (a favourite expression) between veridical and non-veridical experiences, then how can you tell, at any given time, whether your perceptual experience is veridical or non-veridical? Plato expressed the problem very well in the *Theaetetus:*

"*Socrates.* A question which I think that you must often have heard persons ask: How can you determine whether at this moment we are sleeping, and all our thoughts are a dream; or whether we are awake, and talking to one another in the waking state?

"*Theaetetus.* Indeed, Socrates, I do not know how to prove the one any more than the other, *for in both cases the facts precisely correspond;* and there is no difficulty in supposing that during all this discussion we have been talking to one another in a dream; and when in a dream we seem to be narrating dreams, the resemblance of the two states is quite astonishing."[1]

The philosophically accepted answer to the question, how can you tell whether a given perceptual experience is veridical or non-veridical, is that you *cannot* tell, *at the time you are having the perceptual experience* in question. (Some philosophers would say that you can tell *afterwards*, by seeing how the experience in question is related to subsequent experiences.) And this view helps to produce the theory that the truth of an empirical statement is never certain. The answer I propose to make to the question, Can you at the time you are having a perceptual experience, know whether that experience is veridical? is that *sometimes you can and sometimes you can't.* This is a perfectly common-sense answer and, it seems to me, an obviously true one.

Knowing that a certain proposition is true, does not consist in having some peculiar sort of feeling. You *know* that p is true if, first, you have evidence of a certain sort for accepting p, and, second, p is true. The difference between knowing that p is probably true, and knowing that p is certainly true, is that your evidence in the latter case must be of a higher sort, *i.e.* must be more conclusive; in the latter case, furthermore, p must be true.

Now what I am saying is that sometimes when you are having a perceptual experience, you have conclusive evidence that it is veridical; sometimes you do not have conclusive evidence that it is veridical; and sometimes you have conclusive evidence that it is not veridical. If, for example, you have just entered an unfamiliar room for the first time, and have not had a chance to

[1] *Plato,* "Selections" (ed. by R. Demos), Scribners, p. 319. My italics.

find out whether there is a doorway or a mirror in the wall facing you—then if it should *look* as if it were someone approaching you from the front, you would not *know* whether someone was actually approaching from the front. But if in the next few moments you were able to inspect the wall, to see that there was a mirror in it, not a door, and to look about you, then you would know that no one was approaching you from the front, but that someone was approaching from behind. In those few moments you were able to obtain conclusive evidence that no one was approaching from the front.

If you have some astonishing perceptual experience, such as seeing a sudden wind hurl a house into the air, you may actually be in doubt, for a moment, as to whether you are having a dream or an hallucination. But if you collect your thoughts and look about you; get the feel of your body, and notice that your sensory reactions are normal and in agreement; observe that your surroundings (other than the house) are what they should be, your recollections being what they are; see that other people are behaving in a way which shows that they are having perceptual experiences similar to yours—then you will know that you are actually seeing a house being thrown about in the air. During the course of this perceptual experience, you would have collected conclusive evidence that it was veridical.

One question which causes some trouble in this connexion is, *how long* does one " perception ", or one " perceptual experience " last? When you see a man walk across the room, is that *one* perceptual experience, or a series of perceptual experiences? Or if you look slowly around a room, noticing the furniture and observing the people in the room, is that one perception or several perceptions? Can a single perceptual experience last several seconds or even minutes? or can it last no longer than a fraction of a second? This is a question as to how we are to use the expressions " perception " or " perceptual experience ". But the answer to it determines in part the answer to our main question. For if we say that a perceptual experience may last several seconds or even minutes, then it is the case with regard to many a perceptual experience, that it contains its own verification within itself. That is to say, one often obtains conclusive evidence that a perceptual experience is veridical, during the course of that very perceptual experience. While if we say that a perceptual experience can last no longer than a fraction of a second, then it is perhaps the case that no one ever obtains conclusive evidence that a perceptual experience is veridical, during the course of that experience. But it does not follow that one

could not know that a given perceptual experience was veridical, at the time one was having it. For you might easily have obtained, *previous* to the occurrence of the perception in question, conclusive evidence that the perception was veridical. For example, you might conclusively verify, by examining it, that the thing in the corner which looked at first like a chest of drawers, is really a radio. Having once established the fact that it is a radio, if you looked at it again a moment later, you would know that you were really seeing a radio.

In whichever of these two ways one uses the phrase " perceptual experience ", therefore, it is the case that one can know that a given perceptual experience is veridical, at the time one is having the experience. That is to say, it is possible for you to know with certainty, when you are seeing an elephant, that you do see an elephant ; and it is possible for you to know with certainty, when you are hearing a dog's bark, that you do hear a dog's bark. It may be said that it is *logically* possible, in each such case, that you are mistaken. And this is true. But it does not, in the least, follow from the fact that it is logically possible that you are mistaken, that you do not know with certainty that you are not mistaken.

Let us consider the possibility that two perceptual experiences, one veridical, the other non-veridical, may be " intrinsically similar " ; or, to put it as Plato did, it may be that in both cases " the facts precisely correspond ". This is supposed to raise a difficulty as to how one can know whether a given experience is veridical or non-veridical. It seems to me that the difficulty arises because the phrases " the facts precisely correspond " and " intrinsically similar " are confusing. Suppose that it looks to you as if there were a man in front of you walking towards you. Then you discover that it is not a door you are facing but a mirror, and that the man is approaching from the rear. Suppose that a few moments later, the man walks towards you again as you are facing the mirror. Your visual experience is the same as in the preceding case, but this time you know that you are facing a mirror, not a doorway ; you have evidence in your possession, by virtue of which you know that no one is approaching you from the front.

Are we to say that in both cases " the facts precisely correspond " ? Is the fact that you possess certain evidence in the second case, which you did not possess in the former case, one of " the facts " ? Are the two cases " intrinsically similar " ? This is a question as to how we shall use the phrases in question. If

we say that the fact that you possess evidence that a perceptual experience is veridical or non-veridical is a part of the " intrinsic character " of that perceptual experience—then it follows that if a certain perceptual experience is known to be veridical it is not " intrinsically similar " to any perceptual experience which is not known to be veridical. While if we say that the fact that you possess such evidence is not a part of the " intrinsic character " of the perceptual experience in question—then it may be the case, with regard to two " intrinsically similar " perceptual experiences, that one is known to be veridical at the time it is being had, while the other is not known to be veridical, and is perhaps even illusory.

The main outline of this paper may be briefly summarised as follows :—

The doctrine that empirical statements are never known with certainty has been held by various philosophers, because these philosophers have been led to attach a self-contradictory meaning to the expression " it is certain that ", as applied to empirical statements. They have held, in other words, that there is no proper sense of the phrase " it is certain that ", in which it applies to empirical statements ; and we have seen that this is false. Another important source of this philosophical doctrine has been the tendency to confuse " logical possibility " with " empirical possibility ". We considered the doctrine is a recommendation to restrict the application of " it is certain that " to *a priori* statements, and to sense-statements ; but we saw how pointless and self-defeating it would be to adopt that recommendation. We noticed that a main source of the doctrine is the desire to point out that the differences in the nature of the evidence for empirical statements which are certain, and empirical statements which are only probable, are only differences of degree. Finally, we saw that another chief source of the doctrine is a puzzlement over the question as to whether you can tell at the time that you are having a perceptual experience, whether that perceptual experience is veridical.

III.—MEMORY.

By R. F. Harrod.

It is the purpose of this paper to consider the trustworthiness of memory. This branch of cognitive experience seems to have suffered some neglect at the hands of logicians.

The ordinary man holds that at least some of his memories seem so compelling in their information that it would be scepticism run mad to challenge their credentials. But this very intolerance of doubt should put the logician on his guard. Overweening assurance in cognitive matters is always suspect.

It is important to distinguish two problems with regard to the cognitive nature of memory, although they are inter-related. (1) Is the information conveyed in memory a matter of certainty or only of opinion? Ordinary men and many philosophers would join in holding that at least some memories give certainty. An argument will be developed against the absolutely indubitable character of any memory.

(2) There is the more subtle question whether the memory tells its own story; whether, that is, we can trust implicitly in it as and when it occurs without seeking the support of further experience before giving it full confidence. Do we trust memory because there are reasons external to it for doing so, or is it, so to say, self-authenticating?

Here again the ordinary man would make, on behalf anyhow of some of his memories, a plea that they are self-authenticating. That I entered this room, that I passed along a certain street, have lived in a certain town, been educated at a certain school, been familiarly called by a certain name, these and many things seem so indubitable, as I remember them now, that no further quest for confirmation seems likely to serve a useful purpose. Of course there may be ambiguities in the terms with which these past experiences are described, in the meaning of the words, house, street, school, name and so on. But that I did go through certain experiences, of which the descriptions may give an incomplete definition, nothing can persuade me to doubt.

It is the purpose of this paper to raise such a doubt, and to plead that the opinion that memory is informative is a hypothesis which must take its chance in the rough and tumble of experience along with other hypotheses.

This paper is divided into three parts. (1) There is an analysis of the nature of memory as it presents itself in our experience, which is preliminary to the discussion of its informativeness. (2) There is a brief discussion of the status of various kinds of memory in relation to certainty and opinion. (3) The view that the informative character of memory is a hypothesis is examined, together with the grounds for believing the hypothesis to be correct.

I.

I propose to start from the total present experience of an individual who may in sophisticated language be said to be sitting in a room and to remember having walked along a street. That total experience is shot through with interpretation, inference and opinion. He cannot look at a chair without at the same time thinking of its structure and qualities in a way that is only possible owing to past experiences of similar objects. Since we are inquiring into the validity of memory, it is necessary to make a drastic abstraction and think away all those elements in his present experience which are the result of past experience and inference. Opinions about the external world with its bodies and laws of nature cannot be come by save by that collation of our past and present experience which memory alone renders possible. No validity must be claimed for these opinions while the claim of memory to be truly informative is still *sub judice*.

The totality of the experience may be divided for simplicity into two parts, the sense-impressions connected with the act of sitting in the room, and the memory of having walked in the street. Each part is itself a whole of parts with a complex structure. There are some resemblances between elements in each structure; each structure contains items with colour, shape, size and so on. These resemblances enable the memory to be distinguished from the non-memory, because each element in the memory that resembles an element in the non-memory differs from its correspondent in a similar way. Thus seeing a red object, say, a cushion, differs from remembering seeing a red object, say a pillar-box, in a way similar to that in which seeing a man standing in the room differs from remembering seeing a man standing in the street, and to that in which having a headache now differs from remembering having had one in the street. This resemblance with a difference would be still more striking if we supposed the person sitting in the room to remember having sat in it on a previous occasion in precisely similar circumstances.

It would be wrong to emphasise the resemblance too strongly. Opinions differ as to whether, when the memory of, say, a sensory experience occurs, the memory is composed of sub-sensory elements each resembling its original in a faded way. Alternatively, it may be that the elements in the memory event are purely symbolic, that when for instance some one having seen several red things comes to remember seeing some red thing, it is the word red and not a faded replica of the colour red that is present in the memory structure. None the less, the relation between the two kinds of red may be present to the rememberer.

Let us now consider the question what information about the world is vouchsafed by the present total experience. What is necessary to give a complete record of it? There are certain sensory elements, coloured shapes, light and shade, internal feelings and so on. It is sometimes said that these are directly known. They are certainly occurring. Whether it adds anything to say that they are "known" seems doubtful. If the record is to be complete, all the shapes and shades and their mutual relations must be set down in detail either on a map or by any appropriate set of symbols. Then the whole structure of the memory with all its terms and relations must be set down, for that is certainly part of the present total experience.

It may be stressed in passing that there is a complex structure in the memory event. It has been argued that this is a mistake; that all the complexity is in the remembered event, and that the memory is a simple unitary act of the mind apprehending a complex. This view is surely untenable. Take the case of a mistaken memory or, should we say, of a state simulating memory, of a street with houses, gardens and traffic. Here the complex must be in the memory event, for there is no remembered event. Will it be said that a genuine memory is less articulated than the state simulating it? Then wherein is the simulation? Again, if there is any truth in our view of the past which we have pieced together, not every detail of a given totality is remembered. The material of the houses may be remembered but not the number of storeys, and so on. The memory event must then be as complex as what is said to be remembered, which is a selection of a total experience, for how otherwise can we say how much of it is remembered? There must be as many terms and relations as there are in what is remembered. And these must be properly disposed in relation to one another. It will not do if the paving stones are on the roofs and the slates in the street. In fact, the memory must be in some sense a copy of the thing remembered. This does not involve the alleged fallacy in the copying theory of

truth, because that theory postulates that the mind is aware of a copy rather than a reality, and so introduces an otiose *tertium quid*. In this account the memory event is merely said to constitute or contain a copy of what is supposed to be remembered. Finally, to postulate that the structure as a whole is a copy does not prejudge the question whether each element has any sensory similarity to the remembered elements.

In drawing up the record, then, the complex of present sensory elements must be set out and the complex structure of the memory event. Is there anything else in the world which this experience entitles us to record? Must we not also record in all its detail the event remembered? A memory is not identical with the experience remembered. Does this present memory enable us to specify some part of the world lying outside the present totality, namely the remembered event? That is the question to be discussed.

It must be admitted that the differentiation of memory from other parts of a present experience has been rather hastily characterised. Suppose that instead of a memory of walking down a street, there was an imaginative picture of walking down the street. This need not consist of mental images. I may suppose myself walking down a street past a red letter box, the letters R—E—D symbolising that colour in my mind. All that was previously said before about the resemblance with a difference between elements in the memory and elements in the experience of sitting in the room may now be asserted of the resemblance between elements in my imagined excursion and those in the experience of sitting in the room.

In fact, it may be that the clue given by resemblance with a difference does not indicate the distinction between sense experience and memory but between sense experience and a wider range of other experiences including day-dreams and reveries, as well as memories. The difference between these two kinds of experience is directly presented as well as the resemblance of certain components of one class to components of the other. Is it permitted to designate without more ado the difference between these resembling components by calling one lively and the other a faint counterpart?

It is to be emphasised that these terms must not be interpreted literally. In the ordinary sense of lively the memory may be much more lively than the non-memory. For instance, the experience of being in this room that I remember may have been charged by an emotion illuminating the scene with a vivid excitement that even now re-kindles in my memory while I am sitting

listlessly in my chair dead to the world around me. The words are to be taken as terms of art used to distinguish a quality of experience which is directly recognised. Again, this usage must not be taken to imply that the component of memory is a faded image of a sensory experience nor to exclude the possibility that the components of memory are arbitrary symbols somehow representing the remembered elements.

This usage enables us to define shortly the difference between memory and reverie. When what we call a memory occurs, the structure is accompanied by a propensity to predicate liveliness of it, although it is not, in fact, lively in the present totality. This may be regarded as the sole and sufficient characteristic required to distinguish memory from reverie. The propensity may not always be strong, indeed, it may be so weak that its presence is a matter of doubt, there may be an indeterminate borderland between memory and day-dream. This possibility has no tendency to impede the arguments about memory which will be developed.

The foregoing paragraph introduced a sophisticated term. It was said that in memory we have a propensity to " predicate " liveliness of the memory structure. It would be undesirable to introduce such a sophistication at this stage, save by necessity. Could we not simply say that there is a propensity to " conjoin " liveliness with the structure ? In the imagination there are many permutations and combinations of elements. Why should not liveliness be an attribute that may be conjoined with others ? The answer is clearly simple. If liveliness were conjoined to the memory structure, that structure would not be faint ; it would lose the character which by definition it is recognised to have.

Why, it might be objected, should not the faint structure and the same structure conjoined with liveliness exist side by side ? But this conflicts with experience, if side by side is used in the sense in which the memory of walking in the street and the lively experience of sitting in the room are experienced as side by side. If the validity of memory is fully admitted, and the lively experience of walking in the street is to be put on the record, it must not be placed on the record alongside the lively experience of sitting in the room, and the memory of walking in the street, but above (or below) the record of those two structures. There is a sense in which sitting in the room and remembering the walk are alongside, and remembering the walk and performing the walk are not, although these last mentioned may be regarded as alongside each other in their own different way. A two-dimensional surface provides a convenient medium for symbolising these mutual relations.

If, however, we set alongside the lively experience of sitting in the room the faint counterpart of a walk in the street and that only, we have omitted something that is also alongside the lively experience of sitting in precisely the same sense that the faint counterpart of a walk is alongside it, namely the tendency to predicate liveliness of the walk structure. Hence, it is quite clear that the predication of liveliness of a structure is to be distinguished from the conjoining of liveliness to it. It is not to be supposed that liveliness was conjoined to the walk in the street twice over; one such conjunction is already represented above (or below) the experience of sitting in the room; it is impossible to place another alongside it; yet the record of the faint counterpart of a walk is insufficient. Some such notion as predication must therefore be accepted as necessary to an adequate record of all that occurs alongside the sitting in the room.

It is sometimes said that we have direct knowledge of the data which occur in a sensible experience; *e.g.* that we know that that red patch is square. Such a form of words may have meaning, though I must confess that I have never apprehended what it is. What at least is clear is that the knowledge constituted by the predication of liveliness of the walk structure, if indeed that predication is finally held to be valid, is totally different from the so-called knowledge that that red patch is square. The view that knowledge is properly employed in both cases cannot be saved by saying that they are different species of a cognitive relation. It is of the essence of a sensible experience of a red square patch that red and square are, in fact, conjoined within the experience, whereas it appears to be of the essence of the predicating experience just described that liveliness and the walk structure are not so conjoined. What two kinds of experience could be more opposed? It may be said that the predication does in a sense link liveliness to the walk structure. But this link is different from and inconsistent with the kind of link or conjunction which happens when a lively walk happens or a red square patch is experienced. And it is just because of that difference that we had to introduce this novel concept of predication. Where the conjunctions of nature end, those of knowledge begin.

In the description of memory here set forth, no mention has been made of the past. This need not be introduced as an additional ultimate concept. We have already seen that the lively walk, if it exists, is not alongside the lively sitting, the faint counterpart of the walk or the propensity to predicate liveliness of the walk structure in the sense in which those three

experiences are alongside each other. The past may be defined simply as the *Lebensraum* of the lively walk. If the propensity to predicate involved in memory did not vouch for the existence of a lively counterpart, no meaning would be left in the idea of a past.

The chain of past events of which we commonly think can be constructed with the aid of memories. Thus, if the memory of A is remembered as part of an experience of which lively B was an ingredient, lively A has to be represented further above (or below) the present than B.[1]

II.

Memory has been distinguished from imaginative reverie by inclusion of a propensity to predicate. In some cases, it may be said, or, according to one idea, in all cases, there is an actual predication. It is consistent with the foregoing arguments to make propensity to predicate cover the case of actual predication, when that occurs, or even to substitute actual predication for propensity to predicate. It might be said that the remembering state may either be one of certainty or of opinion. Or, alternatively, it may be asserted that it can only be one of certainty.

It is not necessary to argue about terminology. Some would hold that when a man says " I remember X " he ought to mean " I know X ", and that when his state is one of opinion only he ought to say " I think I remember X ". The significance of both states requires logical scrutiny.

According to the ordinary view, even when there is some doubt in the mind, the memory is not wholly lacking in cognitive character. When a man thinks he remembers something, it is supposed that in many, though not all, cases that something happened. The frequency of occurrence may be roughly correlated with the strength of the opinion. Even when the propensity is very weak, if on the occasions of such weak propensity occurring, the event has sometimes happened, the memory is not without cognitive significance. For since the event remembered is usually even in the simplest cases highly complex, *e.g.* the configuration of the street down which I walked, it is very improbable that such a memory should correspond to a lively event by chance.

Turning now to the narrower class where there is no question

[1] Of course sophisticated man peoples the past with many events of which he has no memory ; but then he also peoples the present with many events of which he has no direct experience.

of doubt in the mind, we have to consider the view that all memories claiming to be certain are indeed so. Is there a class of memories, distinguishable from memory-opinions when they occur, of which it is possible to say that if one of this class occurs the remembered event happened? But is it not the case that memory often plays us tricks? Some may be disposed to rebut this count against memory by distinguishing even in the class where there is no explicit doubt between memories and memories. There is the casual fleeting memory, or, as it may be called, the state which simulates memory, and there is the assured feeling of certitude on which after full reflexion and deliberately testing every manner of doubt we are prepared to take our stand.

This rebuttal is not altogether satisfactory. There is an awkward suspicion that the clearly marked boundary that one would expect to delimit something so absolute as knowledge is lacking. It would probably have to be admitted that some people are quite incorrigible and incapable of applying sufficient tests to their own *prima facie* feelings of assurance. Is certitude to be confined to a small band of apostles of righteousness? And is it not rather dubious to suppose that the cognitive character of an experience changes in kind correlatively with the increase in the degree of a psychological concomitant, attention?

Again, this point of view seems to relegate too much to the scrap heap. It safeguards the validity of the small class of elect memories. But is there not much to be learnt from those experiences which, since they cannot be implicitly relied on, are by this account merely states simulating memory? They are, indeed, often misleading, but we should be hard put to it if we had to guide our lives without their aid.

At this point we may summon to the assistance of memory the fact, which cannot be challenged, that it is only on the basis of opinions which depend on trust in some memories that we can say that memory ever plays us tricks. The more sure we feel that seemingly reliable memories are sometimes wrong, the more reliance do we thereby place on the view that a great many memories have been correct. If it could be shown that all memories subsequently judged misleading could be placed at the time of their occurrence into the inferior class of mere semblances, then this argument would do much to vindicate the validity of memory. But it is by no means certain that the misleading memories can always be detected as inferior at the time of their occurrence.

It is quite true that if we are completely sceptical with regard to the validity of all memories, we shall have no grounds for

condemning some as misleading. But this does not save us from an awkward dilemma. Let A, B, C . . . X be a series of facts vouchsafed by memory, of which X is presented with equal assurance but inconsistent with the others. Our natural disposition is to accept A to W but to reject X. Now if our unwillingness to accept X impugns the validity of memory itself, we have no specific grounds for rejecting X, since we can no longer take A, B, C, etc., as true. None the less, the inconsistency of X with the others does give grounds for impugning memory. The proposition that memory is valid entails A to X. But X is inconsistent with A—W. The only way out of this dilemma is to reject the proposition that memory is valid. Then there is no inconsistency; we have no grounds for believing that either A—W or X are true, though either, but not both, may be.

This conclusion is not unavoidable. For the rejection of X on the grounds of A—W implies some intermediate general proposition about the nature of the world, unless at least X is the direct negative of one of the memories A—W, and it is not necessary to suppose that memories which directly contradict each other ever exist together. Thus I may have a large number of reliable memories which imply that a friend died last month. X may consist of a memory which seems equally reliable that I met him in the street yesterday. The intermediate proposition is that it is impossible to meet a man in the street who died a month ago. But this is not necessarily so. We have only to revise our ideas sufficiently about what is possible in the world to make all memories reconcilable. In fact, we are not willing to do this, and prefer to reject a memory, even though it has the highest degree of cogency ever experienced when it occurs, rather than make a radical revision of our ideas of the order of the universe based on a large number of other memories.

All these difficulties and inconsistencies may be overcome by a simple revision of the definition of the cognitive nature of memory even of the highest status. For the proposition that, when a memory of the highest status occurs, we know that a lively counterpart of its content has occurred, it is only necessary to substitute the proposition that when a memory occurs, it is probable that a lively counterpart of its content, or something similar to it, has occurred.

This substitution has the following advantages :—

1. It destroys the necessity for having a rigid and unplausible boundary line between memories proper and states simulating memories.

2. It is consistent with some vagueness about the distinction between memories and day-dreams or dreams.
3. It justifies our propensity to reject a memory, even if it appears to have the highest reliability when it occurs, rather than reconstruct our view of the universe based on many other memories.
4. The misleading character of a memory, even of the highest status, no longer undermines the reliability of memory generally.
5. It seems reasonable to expect to find correlation between the degree of reliance which the subject is disposed to place upon a given memory and the frequency with which other evidence, when that is available, points to the lively content having occurred; and this correlation is, in fact, found.
6. It also seems reasonable to suppose that in certain cases the probability vouchsafed by memory is so high as to be approximately equivalent to certainty.

It is to be noticed that this revised definition requires some change in widely accepted views about the nature of direct knowledge. It is usually held that the data supplied by direct knowledge are matters of certainty, and that probability only attends the inferences drawn from them.[1] It is now claimed that the proper way of recording certain data provided directly by our experience without the aid of reasoning is to state that an event of a certain composition may have, or is likely to have, occurred. Memory is, in fact, held to be informative, but not infallible. It is an example of a direct cognitive relation, but lacks certainty.

This view overcomes the difficulties due to the existence of misleading memories, it harmonises with what we believe about the working of other parts of the natural order, and, if prejudices arising out of traditional logic can be overcome, it should appear sensible, and even intellectually satisfying.

But though the view is sensible enough, it is still necessary to ask whether there are any grounds whatever for holding it. What reason have we for supposing that memory is informative at all? This is the crucial question.

[1] *Cf.* the first sentences in *A Treatise on Probability*, by Mr. J. M. Keynes. "Part of our knowledge we obtain direct; and part by argument. The theory of Probability is concerned with that part which we obtain by argument."

III.

It may be objected that this is pushing Cartesian scepticism too far. Everyone will be able to think of events for which memory seems to vouch with such absolute assurance, that he will be entirely impatient of any doubt. To question his convictions may seem to him to be degrading philosophy to a futile game. Or he may put it: "it is necessary to begin somewhere; these memories seem to give the greatest degree of assurance man has ever had about anything, or ever hopes to have; if you do not build on them, you will never find anything upon which you can build; if you are determined to believe nothing, you need not; but then you had better leave the constructive tasks of philosophy to others whose minds are not eroded by this childish negativism".

To this plea of common sense, the logician must, I fear, present a stern opposition. Within the present totality there is a structure of faint elements, of which there is an overwhelmingly strong propensity to predicate liveliness. But can this propensity serve instead of a good reason? The propensity might exist, and yet there might, in fact, never have been such a lively structure. The existence of the propensity to predicate cannot be said to entail the existence of the lively structure, or even to entail that the lively structure probably existed.

This clash between common sense and logic having been set forth, it seems impossible to proceed further along this path. Philosophical discussion would soon give place to acrimonious argument.

It seems expedient, therefore, to approach the matter from an entirely different point of view by abandoning completely the self-authenticating character of memory. Let us suppose instead that the view that memories are informative is a mere hypothesis. Is it possible to find verification for it? In answering this, we must remember the position of primitive ignorance with regard to the nature of the world from which this inquiry began. (The hypothesis is that memories are informative only; there will be no further reference to the view that memories are infallible.)

How is support to be found for the hypothesis? Since memory is *sub judice*, and almost all, if not all, that is alleged to be known now is only known on the assumption that the information supplied by memory is correct, this support may be hard to find. The only procedure open to us appears to be to make predictions on the basis of the hypothesis that memory is informative and test the hypothesis by their success or failure.

What is the nature of a prediction? Like memory, prediction predicates liveliness of a given structure, although within the present totality the structure is not, in fact, lively. What is the difference between memory and prediction?

There may be a desire to distinguish between them by asserting that memory is direct knowledge and prediction inferential. This, however, will not do, since some inferential judgments predicating liveliness of faint structures relate to the past and prediction is supposed in sophisticated language to refer to the future. What is this future?

It has already been shown how memories enable a chain to be constructed termininating in the present totality. This is the case whether their supposed validity is chimerical or not. Is it possible to suppose quite simply that it is within the scope of imagination to continue this chain, thus providing a *Lebensraum* for the lively contents of predictions? Perhaps the process is a little more roundabout. It is clear that in the imagination various elements of experience may be combined. In memory we have experience of many lively counterparts being accredited to faint contents. By a simple process of analogy or extrapolation a faint counterpart of the lively content of the present totality may be imagined. But where? If a lively structure B is remembered as occurring simultaneously with the memory of A, lively B comes on the chain between lively A and the memory of B. For B in this proposition write a present lively experience. This experience must come, therefore, between lively events now remembered and the supposed memory of the present experience. And so an extension of the chain is postulated on the side of the present opposite to the past. This extension of the chain which contains the imagined memory of the present is called the future, and on it room may be found for the lively contents of predictions.

It still remains to consider how a prediction may be distinguished from a memory at the time of its occurrence. It may suffice to say that the propensity is to predicate liveliness of the present faint structure on a part of the chain on which a memory of the present is imagined as possible. Indeed, the prediction may itself be remembered at the same time that it is fulfilled; if this is imagined when the prediction occurs, it follows at once that the conjunction of liveliness with the structure in the prediction is predicated to occur in the future in the sense defined in the foregoing paragraph.

Prediction rests on two fundamental hypotheses, namely (i) that the assertions of memory are informative, and (ii) that

things which have remained similar for some time are likely to continue to do so. *The possibility of verifying a prediction* rests on the hypothesis that memory is informative, for unless the memory of the prediction being made which accompanies the fulfilment of the prediction is informative, there is no reason to suppose that the content of lively event M when it occurs resembles the content of any prediction. If, when lively M occurs and I simultaneously remember prediction M, there is no probability that prediction M occurred, the occurrence of lively M cannot be held to confirm any prediction.

I call attention to the form in which I have stated the fundamental postulate of induction, that things which have remained similar for some time are likely to continue to do so. This appears to me to be the right way of stating it. Primitive man is confronted with an extraordinary degree of stability in nature; the hills, the woods, the stars, the shape of his own body remain in large degree similar through time. Certain changes are continually occurring. The progress of knowledge seems to consist largely in being impressed by the stable elements within the changing panorama. Thus the arrangement of the causes of our sensory experience including our own bodies in a three-dimensional continuum rendered an enormous advance possible in the appreciation of stability. In place of a bewildering series of kaleidoscopic changes as I walk about the room, we have the apprehension of stationary and unchanging chairs and tables. Each discovery of laws of nature involves another advance. The ideal of science is to attain a view of things which renders the whole panorama of nature completely constant. Given the configuration of things at a point of time and the laws of nature postulated, the whole process may be regarded as a continuance of the same state of affairs.

If predictions are based on the two hypotheses: (i) that memories are informative, and (ii) that things which have remained similar for some time are likely to continue to do so, their fulfilment is in agreement with these hypotheses. It is to be observed, however, that if either hypothesis is wrong, the verification gives no positive support to the other. Let us consider each hypothesis separately. Suppose that it is desired to verify the hypothesis that things which have remained similar for some time are likely to continue to do so. If memory is known *a priori* to be informative, it enables us to distinguish the stable from the fleeting elements in experience. We make predictions by extrapolating the stable elements into the future. If by so doing we obtain correct results, there is a strong probability that the

inductive hypothesis which enabled us to do so is correct. If it were incorrect, any one of an infinite number of configurations would have been equally likely to succeed the present experience; consequently it would be very improbable that we should often succeed in predicting the succession which, in fact, occurs. This improbability is the inverse of the probability of the inductive hypothesis established by our success in prediction.

It is to be observed that the predictions here considered are those of everyday in which we rely on elements in our experience already known to have been stable. Predictions designed to test a new hypothesis are in a different class. A new hypothesis is intended to make a revision of our ideas about what the stable elements are, and the success or failure of the prediction only has relevance to the proposed revision. But the predictions of everyday, which tend to confirm the view that things which have been stable will continue to remain so, are much more numerous, the predictions, namely, that the same old streets, houses, fields, sun, moon, etc., will show themselves in response to appropriate actions on our part.

But if memory is not known to be informative, the alleged verification of the prediction gives no support for the inductive hypothesis for two reasons. (1) There was no valid way when the prediction was made for distinguishing things which had remained similar from those which had not. (2) There is no reason to suppose that a given event was, in fact, predicted.

Now suppose that it is desired to verify the hypothesis that memory is informative. If the inductive hypothesis is known to be correct on independent grounds, the fulfilment of a prediction supports the view that memory is informative. For the fulfilment is *pro tanto* evidence that the stable elements in the situation have been correctly selected. But that selection was based on the hypothesis that memory is informative, for without the aid of past experience it would be impossible to distinguish the stable from the fleeting elements in a given totality. Therefore, if the selection proves correct it supports the view that memory is informative, and a very high degree of probability for the validity of memory may be obtained. This result, however, would only stand if it could be assumed that the memory of the prediction is correct. But this depends on the memory hypothesis the truth of which is at issue. It has been shown that the informativeness of memory plays a double part in the process of proof, and though accepting the inductive hypothesis *a priori*, would tend to establish the informativeness of memories on which predictions were based, if only those predictions could be assumed to be

correctly remembered, as they cannot be, this attempt to establish the informativeness of memory falls to the ground.

If the inductive principle is not established *a priori*, the verification of a prediction, even if that could be remembered, would lend no support to the memory hypothesis. If when the prediction is made there is no reason to suppose that hitherto stable elements will live longer than fleeting elements, the fact that the elements selected as stable are found to endure is no evidence that the memory hypothesis which enabled them to be selected as stable is correct.

Thus the position is that if the validity of memory were known *a priori*, the verifications of predictions would lend strong support to the inductive hypothesis. If the inductive hypothesis were accepted *a priori* the verifications of predictions would lend strong support to the memory hypothesis if only we could be sure independently that predictions were correctly remembered.

If neither hypothesis can be accepted *a priori*, the verifications of predictions are in agreement with the two hypotheses, but lend them no support. What is the position? In the present totality I have faint memory structures, such that if lively counterparts are accredited to them, there appear to be stable elements in nature. I have memories of predictions based on the supposition that those stable elements will continue stable. But I have no reason to suppose that the lively counterparts existed, I have no reason to suppose that the predictions existed, and I have no reason to suppose that stable elements are likely to continue stable. The whole scheme of ideas is mutually consistent, but I have no reason for giving it credence. If I assumed the opposite, no probability would be violated. The whole scheme might well be a product of my fancy here and now. And if that is so, I have no reason whatever to suppose that the universe will not presently dissolve. All prudent men should live on their capital. The rate of interest is far from covering the risk of premature dissolution.

From this impasse it appears to me that we are rescued by two considerations with which I shall deal in turn.

(1) May it not be that the inductive hypothesis can be accepted *a priori*, not merely as a working hypothesis but as a truth? The proposition is that *if* certain things have been found to remain stable for some time, they are likely to continue to do so for a little longer. This proposition is conditional. It is not claimed that stable elements have been found, for the memory hypothesis is still subject to inquiry.

It may be feared that I am endeavouring to re-introduce the

principle of the uniformity of nature in new phraseology. But this is by no means so. I am only claiming a uniformity limited in space and time and scope of application, and I am only claiming probability, not certainty. The general uniformity of nature appears to me a wild and somewhat disreputable speculation of philosophers.

The principle for which I argue can only be established by reference to the general nature of the universe. Of this in a certain sense we know nothing *a priori*. It might be a Heracleitan flux through and through, or it might be uniform through and through, or it might be any form of admixture. But suppose it were possible to discover by experience that it was not Heracleitan through and through, would anything follow? Let us suppose that by experience it was discovered to have certain stable elements in some part of it. Experience only vouches with certainty for that part of it which constitutes the experience. Accepting the experience and turning to review the general constitution of the universe, it would be possible to say of it that it has in a certain part of it stable elements. Now, if contact has been made with certain stable fragments, it is improbable at any time that one is on the extreme edge of those fragments. Whatever their size, it is much more probable that one is at some distance from the edge.

To say that anything is as likely to happen as anything else at a given moment despite experience, is to affirm that immediately outside experience the universe is entirely Heracleitan. If it has not been so within experience, then one must just have finished exploring the whole of a specialised fragment, and this, though possible, is *a priori* improbable.

The principle stated is related to the theory of sampling, and it is suggested that it should stand at the basis of all logic. Of course, as is well known, a sample may be entirely misleading; and this is recognised when it is admitted that the universe may at the next instant dissolve and leave not a wrack behind. In the absence of knowledge to the contrary the sample may be accepted as a guide to what is likely to be in the vicinity.

It is important to emphasise that this principle is not based on experience; only if its validity is independently established, shall we be able to tap the findings of past experience. Within a present, however, we have experience of one kind of continuity —spatial. In reflecting upon this, we may contemplate an example of an application of the principle. A room commonly contains smooth surfaces of various shapes and sizes. Suppose a man's vision, reduced to a pin-point, to move for a pre-determined

finite length over a surface, or alternatively the finger of some one blindfolded. It is improbable that at the end of the time it would rest on the edge of the surface. This proposition is independent of the sizes of the objects and of the distribution of their sizes. There is no assumption of an equally probable chance of each size or of a distribution according to any law. The room may be filled with a chance collection of heterogeneous objects; the probability is valid, however improbable—according to some other principle—is the distribution of sizes; and of course it is assumed that this is entirely unknown. The meaning of this probability principle may here be interpreted in terms of frequency. If a large number of experiments are made, the eye or hand will come to rest on the edge of a surface much less often than at some point a finite distance from the edge.

It may be objected that the experiment will take time to perform. But no experiment is necessary. The truth may be apprehended *a priori* within a given present. No doubt, if the experiment is made the principle will be verified. But, as in the case of some more sophisticated probability propositions, the experiment would be entirely bogus; if carried out, it would only serve to establish the propriety of the conditions, namely that the parties to it had acted in good faith, and not the truth of the probability proposition itself.

There need be no hesitation in passing from spatial to temporal continuities, for the spatial character of the surface plays no part in aiding the mind to apprehend the probability law.

It is with reluctance, that I have introduced this *a priori* principle. I put it forward, however, with some confidence; and would even venture that it may be possible to build much of the structure of logic upon it alone. The temptation to consider its relation to the orthodox theory of probability must be resisted here. Has not too much attention been paid in that theory to the concomitance of characteristics, and too little to the existence of stabilities in nature? The consequence has been an unnecessary and unrealistic assumption of atomism, with its corollary, from which no honest escape is possible, that we can neither know nor reasonably conjecture anything whatever about what is likely to happen next. It is sometimes forgotten that atomism is itself an hypothesis requiring to be justified by evidence. It cannot therefore be adduced in argument against the probability here set out which makes no assumption about the general character of the universe whatever.

Now so far the informative character of memory has not been admitted, and the inductive postulate has been stated in a condi-

tional form. But it has already been shown that if the inductive postulate could be accepted in this form *a priori*, and waiving for the moment the difficulty about remembering predictions, to which I shall return, the memory hypothesis would be strongly supported by the fulfilment of predictions.

If memories are informative, stable elements can be selected. If it is reasonable *a priori* to assume the continued stability of these, predictions can be made. In general, if a prediction is fulfilled it is highly probable that it was made on correct grounds, for any one out of an infinite number of things may happen.[1]

If we start from the position that any one of an infinite number of things may happen at any moment, the fulfilment of the prediction of a particular thing verifies in high degree the grounds of the prediction. Now the grounds for the myriad predictions of everyday life, which are in fact fulfilled, depend on the informative character of memory. For only if memory is informative can we distinguish the stable elements in the universe. Therefore there is very strong empirical support for the informative character of memory.

At an earlier stage, it was held that if either the memory hypothesis or the inductive hypothesis could be established *a priori*, experience would give strong support for the other. Those who cherish that internal feeling of assurance which they have in regard to their memories may have hoped that I should be driven to postulate the informative character of memory *a priori*. In fact, I have done the opposite. It may now be confessed that the alternatives were not quite symmetrical, and that the support given to the inductive hypothesis by postulating the informative character of memory would not have been entirely satisfactory.

The correct results of predictions based on memory would have established the continuity of stable elements in the period falling within the predictions. But a person who was profoundly sceptical of the stability of the universe might contend that nothing was established for the future. At the next moment all would probably change. Indeed, if the validity of memory is accepted, the fulfilment of the predictions really adds little. If the stability revealed by memories stretching, say, for forty years is no evidence of continued stability, the stability revealed for the few extra hours or days during which a prediction is

[1] The idea that fulfilment tends to verify in a high degree the grounds of a prediction sounds at first blush paradoxical. But this is because by prediction is usually meant the choice among a small number of possible alternatives; in this case fulfilment only gives a low degree of support for the grounds of the prediction.

fulfilled is no evidence either. The sceptic must be routed on his own ground by the assertion of the validity of the inductive principle. Once that is done, the memory hypothesis may be established by induction in an unexceptionable manner. It only remains to consider the memory of prediction, for all this theory falls to the ground if at the time of fulfilment there is no reason to suppose that the prediction was ever made.

(2) The second source from which I seek to draw support in defeating scepticism is the specious present. It appears to me that predictions are often made and fulfilled within a specious present, so that there is no need to resort to memory to find instances of predictions fulfilled. For instance, I may predict continuity for the ordinary objects around me and the prediction may be fulfilled within one present totality. And if a flash of lightning occurs, I may predict and experience its end within the same totality. Only if memory is informative should I have grounds for predicting continuity for the chairs and tables and death for the lightning flash; the success of the prediction within the specious present is therefore strong evidence of the informativeness of memory.

In discussing induction, for economy and to avoid vexed questions, I confined myself to the probability that things which had remained similar for some time would continue to do so. In illustrating the richness of the specious present, it may be permitted also to give examples of the fulfilment in it of other predictions which depend on memory being informative, although they involve the use of a more elaborate inductive argument.

For instance, the prediction that a specific act of volition will result in my arm being raised and the raising of my arm may surely lie within a specious present. This confirms the informative character of my memory of the effects of similar acts of volition in the past.

Or, again, I may hear the early notes of a well-remembered tune. My memories teach me that the vocal chords or instruments which produce such noises usually, though not always, continue to the bitter end. And, hark, of all the million and one sounds and noises that might have supervened, just those expected notes come forth. This instance is a good one. For it is generally admitted that if the successive notes of a tune did not lie within the specious present, they could not be recognised as constituting a tune at all.

If these two lines of thought are correct, the case for the empirical basis of our trust in memory is complete, and the logician's qualms when faced with the plain man's petulant "but

I have direct knowledge that I have just walked down the street " may be dispelled.

The vindication of the informative character of memory here set forth may reduce difficulties connected with its causal relation to the events remembered. So long as memory is regarded as a kind of self-authenticating knowledge of past events, vouching by virtue of its own internal nature for the predications made, some very queer relation between the memory and the past has to be assumed. This relation seems out of harmony with the ordinary processes of nature known to us. Now, though there is still more in heaven and earth than falls within our comprehension, and we could not refuse to postulate a quasi-mystical link between the present and past if the facts required it, it is proper to pause and re-examine the ground carefully before doing so. The account which I have given dispenses with the necessity of assuming any such link. The memory may be regarded as a trace left by the lively event, a footprint in the sand, or the resuscitated pain of an old wound. The human constitution reacts sharply to such an occurrence with the propensity to predicate. But the memory itself says nothing and knows nothing. A hypothesis may then be made that memory is informative just as a hypothesis might be suggested by the footprint that the island is inhabited. The hypothesis is found by experience to be highly probable. But long before this verification has been explicitly checked, man's instinctive equipment makes him get busy and think and act as though he knew that the memory was informative. A by-product of this instinct is that philosophers have been led to exaggerate the scope of our intuitive knowledge to the detriment of their own studies.

One more word should be said about the heedless credence given by common sense to memory. There is every reason to suppose that man is endowed with a violently strong propensity to trust to memory, that is, to conjoin liveliness in his imagination to the faint structures thrown up in the present by past lively structures, and to wish to predicate that conjunction as a fact. Trust in memory has been of immense biological advantage. And the instinct in rational man to impose such trust had to be especially violent precisely because, pending his full comprehension of the inductive principle, there was no reason whatever to do so. In many respects man has been much guided by the reason that is strong within him, but if in the early stage of its development he had been led by it to complete scepticism about his memories, he would have been lost. It is sometimes beneficial to have anti-rational impulses strong enough to resist the quiet and gentle

but relentless voice of reason. It is the same note of the primitive in "I *know* I walked down the street" that one hears again in "I *know* our leader is a god". These blemishes of immaturity protect man until he reaches his full stature.

In this case of memory, the anti-rational instinct takes him to the same conclusion that reason reaches by a more circuitous and arduous route. And so there is no need for him to endeavour subsequently to shake off the instinct, even if he could. Indeed, he is fortunate in his endowment, for in the rough interludes of history the finer processes of thought may suffer eclipse. So long as the human constitution is conserved, this instinct will remain and, by its consilience with the processes of reason, serve in their stead.

The Pragmatist holds that what works is true. If the arguments of this paper were wrong, that would probably be about the best status one could find for the veridicity of memory. It would be a depressing view, since on it the environment might very probably change in such a way that trust in memory would "work" no longer, but man would be unlikely to succeed in shuffling out of his instinct, which would then be injurious. So long as we are in the position of holding that nothing about the world can ever be known with any degree of rational assurance, there is a temptation to relapse into the pragmatist philosophy. Jibbing at the extreme opinion that no man is any wiser, nor any act more sensible than another, one may take the view that some assumptions, though without foundation in logic, have in fact worked, and that all we mean by wise and sensible may be defined as efficiency in using the assumptions. Biological considerations may reinforce the arguments in favour of pragmatism. It is the purpose of this paper to suggest an alternative philosophy. While denying that the true may be defined as what works, its contention is not inconsistent with the view that the only ground at the level of common sense for trusting memory is that it does work, and that this trust may be connected with biological survival.

The denial that memory is a form of direct intuition and the view that its claim to be informative is a mere hypothesis subject to the test of experience may seem to some unduly sceptical. Yet in the upshot I claim that it provides the only available refuge from scepticism. On the one hand we are confronted with the deductive school, who rely on intuitions, the apprehension of necessary connexions and demonstrations therefrom and relegate induction to a subordinate place. It has always or, should I say, almost always appeared to me that if that view

were correct, nothing whatever could be known or reasonably conjectured about the world. On the other side are the logical positivists who laud induction to the skies and then explain that valid induction merely means obeying certain arbitrarily selected rules. Dare I hope that my middle position is better founded than either of these extremes ?

IV.—CRITICAL NOTICES.

The Philosophy of David Hume. By N. Kemp Smith, D.Litt., LL.D., F.B.A. London: Macmillan & Co. Ltd., 1941. Pp. xxiv + 568. 25s.

There has been in recent years a remarkable spate of books on Hume, of which the present work is, next to Professor Laird's, the most comprehensive. The widespread interest in the philosopher at the present time may be explained by the affinity between his philosophy and that of the most prominent school of the present day, who like Hume seek to carry empiricism to the utmost tolerable limits and to escape the difficulties involved by falling back on a common-sense knowledge which neither admits of nor requires a rational justification in the logical or philosophical sense of "rational". One cannot, I think, class Professor Kemp Smith with this school, but his interest was presumably heightened by the prevalence to-day of Humean tendencies. He, however, resolutely confines himself to the part of commentator and gives practically no clue to his own opinions on the philosophical topics under discussion. As commentator the author has already won fame for his treatment of Kant, and it goes without saying that his book on Hume may also be recommended to all students. The style and nature of his work is, however, very different from that of his *Commentary on the Critique of Pure Reason*, being much less full of subtleties and very much easier to read. This is no doubt largely due to the difference in the subject matter. One must add that it is less strikingly original in its contentions, though it does make a very valuable contribution to the study of Hume written in a lucid style. The work is divided into four parts. The first gives an account of the influence exercised on Hume by Hutcheson, Newton and Locke, the second is a preliminary summary of Hume's views, the third a more detailed commentary, and the fourth contains some remarks on the relations of Hume's various works besides a final summary of his position. This method of composition involves a considerable amount of repetition, but the principle of preceding a more detailed commentary by a general survey of the philosophy of the author commented on is obviously one which has a good deal to recommend it.

Kemp Smith holds that the main motive and interests of Hume were expressed rather in his ethical work than in Book I of the *Treatise*, which can only be understood adequately if we regard it as a prolegomenon to a defence of "feeling" against reason all

along the line. The "adventurous and difficult enterprise to which Hume had found himself committed" is that of questioning the fundamental assumption that assurance ought always to rest either on direct awareness or on logically cogent evidence, and once he had, as he thought, overthrown this assumption in the theoretical field and given "feeling" its rightful position there, he could, *a fortiori*, dispense with the assumption in dealing with ethical questions. "If knowledge be strictly limited to those relations which are derived from the contemplation of ideas, and if all other judgments (those concerning matters of fact and existence) be taken out of the field of knowledge, and treated as judgments not of knowledge but of belief; and if further it can be shown that belief, as thus distinguished from knowledge, rests always on feeling, and never in ultimate analysis on insight or evidence, the principle illustrated in morals will be strengthened and confirmed by proof of its equal applicability in these other fields" (p. 13). Hume's scepticism, in so far as it goes, was intended to show that belief cannot be accounted for if the above rationalist assumption is maintained and therefore must be explained in another very different manner. On the general handling of this theme I must offer two comments: (1) The doctrine that reason "ought to be slave of the passions" and "ought to be subordinate to our natural beliefs" is a terribly dangerous one, especially to-day, and I think that Kemp Smith ought to have discussed more fully Hume's not altogether successful attempt to distinguish between the non-rational beliefs which we ought and those which we ought not to accept. Obviously any such theory as Hume's is only defensible if such a distinction can be made within it, otherwise we shall have no more justification for accepting the best thought-out scientific belief than for believing the crudest superstition. (2) Kemp Smith rightly points out (pp. 547, 549) that a grave defect in Hume's account is the vagueness and ambiguity of the term "feeling" as used by him, but I think that in order to facilitate an understanding of Hume it would have been desirable to give a fuller analysis of the different senses of the term in question as used by Hume. Kemp Smith maintains that Books II and III of the *Treatise*, which deal with the passions and with morals, were written earlier than Book I, and that Hume's ethical views were very much influenced by Hutcheson (as also his views on space and time which, Kemp Smith points out, are by no means sensationalist in character).

Kemp Smith has no patience with the view that Hume is a sceptic. He rightly insists that Hume did not disbelieve in causality or in the existence of external things but merely contended that these beliefs were incapable of rational justification. Further, he rejects the commonly held view that Hume accepted (and indeed invented) the regularity theory of causation, on the ground that necessity in the shape of a necessary transition in the mind is regarded by Hume as an essential part of the analysis of causation,

and that he still often speaks as if there were an objective, incomprehensible causal connection behind the regular sequence. The two reasons seem to me to contradict each other, though this is Hume's fault rather than his commentator's. On the one hand we must remember that, when Hume asserts that he had "never defended the absurd proposition that a thing could come into being without a cause",[1] he certainly gives no indication that he is using "cause" in any but the ordinary sense of the word, and yet he certainly seems to have held that *in the ordinary sense* "cause" could not be analysed merely in terms of regularities or regularities plus a subjective feeling of necessity in our mind. If he had accepted this analysis it would have been quite clear to him from experience that there were causes, though the difficulty as to what justification we had for thinking they would continue to act in the future as in the past would remain. On the other hand, Hume emphatically asserts that the supposed objective necessity is an illusion due to the ascription to objects of a feeling of constraint in the mind ;[2] and in a passage in Book II of the *Treatise* he indeed ascribes necessity to causation but analyses necessity in terms of regularity.[3] But throughout the first book of the *Treatise* one of the chief difficulties is to decide when Hume is speaking ironically and when he is serious, and there does not seem to be any simple solution to the question what view of causation he held. I think the non-sceptical interpretation of Hume's philosophy in general which Kemp Smith champions, and which I myself favour, is only tenable if we assume that a good deal of Hume's theory of belief is rather a *ballon d'essai* than meant altogether seriously, for the kind of psychological explanation Hume gives would, if taken seriously, imply the illusory character of our beliefs, but probably Kemp Smith would not agree with me here.

Similarly, Kemp Smith has apparently no doubt that as regards the physical world Hume was a realist, though he had given up the idea of justifying logically the realist belief. I prefer this interpretation to that of Professor Price (in his recent book) who is unwilling to take Hume at his word. The modern device of making the best of both worlds by maintaining that our common-sense propositions about physical objects are true, but have to be analysed phenomenalistically, or in some quasi-phenomenalist way, had evidently not occurred to Hume, though it may be traced in Berkeley and Kant. Kemp Smith goes so far as to say that "it can be maintained as a general principle that Hume never denies the existence of any conception which has been the subject of controversy" (p. 254), but, as is made clear in a footnote to the page quoted, this is compatible with a certain amount of re-interpretation of the idea. In the case of the physical world Hume,

[1] Greig, *Letters of David Hume*, I, p. 187, quoted by Kemp Smith, pp. 412-413.

[2] *Treatise*, I, 3, 14.

[3] *Ibid.*, II, 3, 2 ; *cf. Enquiry*, Sect. 8.

however, defines the notion in terms of "distinct" and "continued" existence and gives no indication that "distinct and continued existence" has itself to be analysed in terms of actual and possible human experience or sense-data. In the case of "substance" his re-interpretation is more drastic, but he does not deny the existence of the self or even of physical "substance". Kemp Smith insists further that Hume's theory of belief does not affect his account of knowledge as being certain in itself, though we may be uncertain when we have attained it and though at its best it is restricted to the relations of "ideas". In order to make sense of Hume's account it seems again that one is compelled not to take seriously either his assertion that "knowledge and probability are of such contrary and disagreeing natures, that they cannot well run insensibly into each other, and that because they will not divide, but must be either entirely present, or entirely absent",[1] or his assertion, a few lines further on, that "all knowledge resolves itself into probability". Kemp Smith adopts the latter course, or at least regards the passage as only a temporary aberration on Hume's part (pp. 357 ff.).

But the part of the *Treatise* which is of most fundamental importance, for Kemp Smith at least, is the theory of belief. For this theory is the basis not only of Hume's theoretical philosophy but of his ethics and of his whole outlook on life. By showing that our fundamental and indispensable beliefs could not be justified rationally Hume thought he had shown that we must accept our natural instincts to believe as immune from criticism but not try to rationalise them or regard them as giving intellectual understanding rather than practical assurance. From this position to his view of ethics, as based on and inseparable from the instinctive make-up of human nature and having no *a priori* validity apart from that, it was a short step. The situation is complicated according to Kemp Smith by the fact that there is a conflict not only between reason and our instinctive beliefs but between the instinctive belief in causation, which logically carried out leads to the conclusion that our perceptions are dependent on the mind, and our instinctive belief that external objects continue to exist even when not perceived. In a specially interesting passage (pp. 443 ff.) Kemp Smith points out the analogy between the function of belief in theoretical matters and that of sympathy in connection with the matters treated in Books II and III of the *Treatise* and shows where the analogy breaks down. He insists, however, that Book I requires for its understanding some idea of the content of Books II and III, which were probably composed earlier and which Hume is concerned to justify and to fit into a more general account of belief.

Kemp Smith thinks likewise that the famous and crucial passage which sets forth the doctrine that all our ideas are the copies of

[1] *Treatise*, I, 4, 1 (third paragraph).

previous impressions or combinations of such copies must be re-interpreted in terms of Hume's general theory of belief. For the latter to be maintained it was necessary (*a*) that ideas should be exact copies of impressions, (*b*) that they should differ from them only in respect of "force" or "liveliness". For it is only if these assumptions are made that it is possible to maintain that mere association can, by imparting "liveliness" to an idea of imagination, raise it to the status of a belief in the existence of the object which would give a corresponding impression. This view of belief is assumed in the last two books of the *Treatise*, which, as I must again remind the reader, Kemp Smith regards as earlier than Book I. But at other times Hume, like Locke, takes his stand on the less extreme position that impressions are the materials upon which all our ideas are based. This is the case, Kemp Smith points out, when he is dealing with relations, abstract ideas, space and time, which are rather derived from than copied from impressions. In the opening section of the *Treatise*, where he states his doctrine of the empirical origin of all ideas in its most challenging form, he "has not only abstained from complicating his argument by any premature reference to doctrines which do not at this stage allow of sufficiently clear statement; he has, less happily, also abstained from conveying any warning to his readers that the analysis of experience which he is here giving is very far from complete, and that in addition to impressions and ideas—the materials of experience, which are alone here mentioned—they will be called upon to recognise types of 'object' quite other than impressions and ideas, *viz.*, the objects of knowledge and the objects of belief, and as conditioning these two types of object, certain further factors, such as 'acts of comparison', 'propensions' of the imagination, 'qualities of human nature'. The objects of *knowledge* are the propositions—the universal propositions—in which knowledge finds expression. The objects of *belief* stand in no less striking contrast to our subjective and perishing perceptions: they constitute the independently existing *prevenient* world of the workaday consciousness" (p. 218). The interpretation given seems to me plausible and exonerates Hume from the charge of having based his whole philosophy on a premiss which is both obviously false and totally ungrounded. Since I do not like to believe that so great a thinker could have been so foolish, it comes as a considerable relief to me. But the question how the validity of Hume's arguments is affected and how much exactly would be left of them if he had only used the principle in its milder form might profitably have been more fully discussed.

In his treatment of Hume's ethics Kemp Smith maintains that sympathy "has there the same central position that belief occupies in his treatment of knowledge" (p. 148). There is surely a verbal slip here since, as Kemp Smith himself insisted, Hume keeps knowledge distinct from belief and does not explain knowledge in terms of belief, but presumably what is meant is "the same

central position that belief occupies in the first book in the *Treatise* ", or " occupies in his treatment of theoretical (in distinction from ethical) questions ". Thus the principle of the subordination of our reason to our passions in ethical matters so that ethical propositions are reduced to propositions about human emotions of approval or disapproval corresponds to the principle of the subordination of reason to our natural beliefs in theoretical matters. Kemp Smith succeeds in clearing Hume completely of the charge of psychological hedonism, but it would have been better not to express this by saying " that Hume's teaching is quite definitely non-hedonistic " (p. 163), since " hedonistic " would naturally be taken as referring not only to the doctrine that pleasure is the only object of desire but to the doctrine that pleasure is the only good, a position which seems certainly to have been held by Hume and from the stigma of which Kemp Smith has not exonerated the latter.

Towards the end of the book Kemp Smith gives an interesting and reasonable defence of Hume's personal character against the charge that he was unduly influenced in his work by the desire for fame (pp. 526 ff.), and says a little about the relation of the *Enquiry* to the *Treatise* (pp. 534 ff.). In his final comments he asserts that most of the limitations and defects in Hume's system are due to his failure to grasp the seriousness of the problem of analysis of human experience. In this connection Kemp Smith suggests that a more thorough analysis of belief might have led to results more like those of Kant than those of Hume (p. 553). But he regards Hume as attaining a fair measure of success in his treatment of inductive reasoning, though his theory of belief was only developed by inconsistently treating belief sometimes as a mere " feeling " and sometimes as a genuine cognitive attitude. Kemp Smith, in the " concluding comments ", agrees with the usual tradition in ascribing supreme importance for Hume's philosophy to his atomism, but he has not said nearly as much about this in the main body of the commentary as one would expect from the stress he now lays on it. " From start to finish, tenaciously and uncritically, he [Hume] has held to the assumptions which underlie what has come to be entitled the ' composition ' theory—that there are ' existences ' describable as simple, that these simples are ' more real ' than any of the complexes they serve to compose, and that so long as the simples exist at all they remain unmodified in and throughout all change. For has not Hume maintained that unity, when taken in its strict and proper sense, must exclude all manifoldness of constitution; and does it not therefore follow that the ' compound ' is never other than a name merely for a plurality of simples arbitrarily selected and viewed together, for some subjectively conditioned purpose " [1] (p. 559). From this follows a conclusion not fully recognised by Hume, namely, that " change is to be looked for only on the derivative, somehow less substantial

[1] *Hume's Dialogues concerning Natural Religion*, Clarendon Press, 1935.

level, to which the complexes belong, and shares therefore in the questionable, *problematic* type of existence peculiar to them. Since, however, the compound has no 'identity', change is also as little predicable of it as of any one simple considered apart by itself." Kemp Smith, however, pointed out his frankness in admitting the conflict between his fundamental principles and what we cannot help believing, at least in the question of self-identity (App. to *Treatise*, Bk. III). "Hume begins by assuming that it is the simples which are given, and that it is the complexes which have to be determined; he has ended by recognising, as in his doctrine of the primacy of the vulgar consciousness, that it is the complexes which are immediately given, and that the simples, required for the understanding of them, are also under question." But, while Hume's logic has been a success, his psychology has been a failure (p. 561). His ethics is the most stable part of his system and is, like his theoretical philosophy, characterised by a reliance on the absolute authority of Nature; but "should judgments genuinely cognitive in character have to be recognised as entering into belief —as ultimately, by implication, Hume himself admits is the case —the capital position in his ethics, no less than in his general philosophy, will at once be endangered" (p. 565). Since Kemp Smith has written elsewhere on Hume's view of religion, the latter topic is not discussed in this book.

The work in general is too sound to give me much opportunity for criticism, but I think there are certain respects in which the method of exposition might have been improved, though as regards the two most important points, lucidity and thoroughness, there is little scope for objection. I think, however, that the book is unnecessarily long and that the same content might profitably have been expressed with greater brevity. It also seems to me that too frequent recourse is made to lengthy quotations. It is unfortunate that the index is limited to proper names; and I think it would have been better to give the page references of some other edition besides that of Selby-Bigge (preferably the *Everyman*). There are one or two surprising omissions, *e.g.*, after insisting that Hume's reasons for his unusual treatment of geometry do not lie in sensationalism he gives no real account of Hume's treatment of the subject, but confines himself to quoting at length the authors who had influenced Hume, with hardly any comment. It seems to me also that his account of Hume's ethics is scrappy in places. I make these remarks not because I wish to close the critical notice in a fashion which savours of ingratitude for what Kemp Smith has done for us, but because it is the chief business of a reviewer to suggest ways in which he thinks that the book he is reviewing might have been improved.

A. C. EWING.

The Arts and the Art of Criticism. By T. M. GREENE. Princeton, Princeton University Press. London, H. Milford, 1940. Pp. 690. 30s.

The Arts and the Art of Criticism has a dual intention—the improvement of the philosophy of art and of art criticism. The defect of the critic, it is stated in the Preface, is that he is not sufficiently philosophical, of the philosopher, that he generalises without sufficient or sufficiently explored artistic data. The philosopher of art should have the endowment and the training of the artist and the critic as well as what he too often assumes to be enough, that of the philosopher. That such a thing might be, is the hope and despair of many philosophers of art, and of most of their critics. The author of this book, a professor of philosophy at Princeton University, has tried to overcome some of the natural obstacles in the way of this ideal. He, of course, does not pretend to combine in his own person philosophical and artistic and critical powers; but by co-operation with artists, and especially with critics, in the fields of music, the dance, architecture, sculpture, painting and literature he hopes that he has in some measure overcome the handicaps of the mere philosopher. With the exception of the excellent supplementary essay on Beethoven's Third Symphony by Professor Roy Dickinson Welch, the co-operative effort of the various specialists of the Princeton University faculty does not take the form of joint publication. Professor Greene assumes full responsibility for the entire volume—it runs to 506 pages of letterpress with 160 pages of excellent collotype illustration—for it was he "who expressed their findings in the form in which they appear". The book is divided into four parts, each engaged in defining, relating, illustrating and applying categories common to all the arts, traditional and additional categories. It is art criticism, in the outcome, that is to be most definitely served. The final section makes use of the categories, as interpreted in the preceding section, to discriminate "five levels of critical appraisal,"—pure formal beauty, artistic quality or perfection, artistic integrity, artistic truth, artistic greatness, and displays these in a "hierarchical" relationship. In so far as they accept this or a similar arrangement, critics can both pass judgment on works of art at any one of the lower levels, and also assign degrees of value according to the level reached.

However convenient this may be for orderly appreciation and criticism, the reader with special philosophical interests will find that philosophical interpretation of the concepts evoked and put to use is far from sufficient—unless he is content to accept them as defined, or as instruments that are simply to prove their worth in use. The author anticipates this criticism; he states in his preface that he is "concerned only with the formulation and documentation of basic artistic categories and artistic principles". Philosophy is to have a minor rôle, its problems "postponed to another occasion".

The present volume " should be regarded as a propaedeutic to more formal philosophical speculation, for it investigates what most philosophers ignore and stops where most philosophers begin their philosophical inquiries " (x). Yet in the same paragraph it is stated that " an inquiry of this type must, of course, rest on certain philosophical presuppositions, and its findings must entail certain metaphysical and epistemological implications ". Nor is the desire to avoid philosophical problems in accord with his insistence that artists are philosophers, philosophers appreciators of beauty, though for understanding and evaluating art each is not sufficiently what he is not primarily. When the categories employed are of such basic character to philosophy in general and to the philosophy of art as are the categories of matter, form, content, analysis, elements, parts, wholes, philosophy will keep creeping in, in more than a minor rôle. Certainly definition, application, of such categories as these is acceptable in the work of a philosopher only to the accompaniment of some forthright attack on the philosophical issues involved. Otherwise these issues are settled arbitrarily—implicitly or explicitly. The author certainly takes sides on certain philosophical issues, general, and special to æsthetic theory, and the defence of these interpretations being postponed, the reviewer of the present book is in an awkward dilemma whether he attempts agreement or dissent.

Professor Greene's initial thesis is that a work of art as an " organic whole ", " reveals itself only to synthetic appreciation " and that such appreciation must rest on " analytical realisation " of its various aspects and ingredients. To this end the philosopher, as assistant to art and to art critics, must set himself the task of supplying " powerful abstract categories, rigorously defined " (30). Such an abstract category " may be defined as a concept which is essential to artistic inquiry and in terms of which alone art can be systematically explored " (30). Some of these categories are more basic than others in that " they refer to characteristics which every work of art must, by definition, possess. ' Matter ', ' form ' and ' content ' are categories of this type " (31). The ' matter ' of a work of art is that in it which has been expressively organised. The ' form ' of a work of art is the expressive organisation of its matter. The ' content ' of a work of art is that which finds artistic expression through such formal organisation of its matter ", form being the " vehicle " of content (32). The concept of matter is given more technical character when, while still remaining matter, it is developed into the concepts of raw material and medium, each of these having a primary and a secondary form. " The primary raw material of any art is the sensuous (in the case of literature, the partly sensuous) material which, once it has been organised into a language or vehicle of artistic expression and communication, constitutes its primary artistic medium. The secondary raw material of any art is its potential subject-matter prior to all pre-artistic selection and appraisal: its secondary artistic medium is this same

subject-matter after it has been scrutinised with an eye to its immediate availability for the art in question" (39). (Throughout this very special use of the term medium to refer to the work one needs to be alert not to forget subject matter.)

The six chapters of Part I, "The Artistic Categories and the Matter of Art", are devoted to the "matter" of art, and five of these consider separately the "matter" of the major arts, of music, the dance, architecture, sculpture and painting, and literature. The "matter" of each of these is surveyed with full and systematic illustration in terms of raw material, primary and secondary, of medium, primary and secondary. Of special interest is the account of the development of the primary artistic medium from the primary raw material, of the secondary artistic medium from the secondary raw material. The subject-matter of the secondary raw material and secondary medium is both representational and non-representational. Sensuous material, raw and in its secondary form as primary medium, subject matter, potential and selected, are pre-artistic stuff, as contrasted with the "form" and the "content" of art (treated respectively in Parts II and III).

What will have to be a very brief outline of the analysis of the "matter of music" with a still briefer account of that of literature is all that is possible in a review. It will do little justice to the original, the virtue of which is, as the author intends, a display of the varied and subtly selected materials out of which works of art are made, and which are "analytically present in them". "The primary raw material of pure music is auditory sound with variations of pitch, timbre, intensity, and duration, plus silence, regarded as the mere absence of such sound. Tempo, conceived in purely physical terms as rate of speed, should also be included, since it provides the physical basis for tempo in a musical sense, and, in combination with duration, intensity, and other factors, for musical rhythm" (46). "The secondary raw material of pure music (pure is non-representational music as distinguished from program and occasional music) is all the emotions and conative states which might conceivably find expression in pure music" (46). (*As expressed* in music, these emotive-conative states constitute its "content" dealt with in Part III.) The primary artistic medium of pure music has three essential factors—tonal relations of pitch expressed in a scale; the qualitative characteristics of the timbre of different instruments and different human voices; and the quantitative variations of intensity, duration, and tempo. The secondary artistic medium is all those emotive-conative states—the secondary raw material—which might conceivably be given interpretative musical expression, "transformed into a secondary artistic medium through the creative artist's selective apprehension of these states in their more universal aspects" (58). It might be well to interpolate here that the author uses "universal" at times to refer to common characteristics of emotive-conative states (and other kinds of subject-matter), and

again to refer to the " ideal " forms of such states, and again as those experienced, by the musician for instance, when " in proportion to the profundity of his character and insight, [he] is able to experience more vividly and deeply than the normal individual what all men experience in some measure and perhaps only on rare occasions, and to realise, during these experiences of his, their universal human character and import. His own emotive-conative states, taken as typical not of man's usual experience but of what all men, suitably endowed, might experience at their best, thus become the artistic ' subject-matter ' or secondary medium of his art " (60).

In literature there can be little limitation of raw or selected subject matter—*i.e.*, secondary matter and secondary medium ; the generic secondary raw material of literature includes " the whole range of man's inner and outer experience, and all the objects of inner and outer awareness " (117). Its secondary medium, is " much more extensive and varied than that of artists in the other primary media, for it includes the artistically relevant aspects of (i) physical objects and events, and their perceptual attributes, (ii) man's social activities, (iii) his emotive-conative states, (iv) the entire range of concepts and images in terms of which he reflects upon his own inner experiences and his relations to his fellowmen, the world of nature, and Deity " (118). In each art, transformation of the secondary raw material into secondary artistic medium is described as a selection on the part of the artist of what has " immediate availability ", recognised as thus available " before or during the act of creation ". It is not easy to see what can be meant by *immediate* availability ; it would seem to push pretty close to artistic fact. But Professor Greene's account of the " matter " of the arts will not permit of this assimilation—for raw matter and media alike are matter only, are pre-artistic, though the primary medium " determines in important ways the distinctive character of works of art ", and the secondary medium " conditions the truth and significance of artistic content in essential ways ". But in themselves neither primary nor secondary artistic media possess " actual artistic merit " (119). Certainly they are not " works of art "—but that would seem obvious.

Part II deals with artistic form, the " peculiar locus of artistic quality ". In his full discussion of form, Professor Greene includes both the generic patterns which works of art may share and specific form, the unique form of a particular work of art. Form has, he maintains, artistic value in itself and is an end in itself, but " it is also and essentially a means, in fact the only means, whereby the artist can express himself and communicate his ideas to others " (123). In the final chapter of the book, it is placed on the lowest level of artistic worth as enjoyed in itself. But it is the necessary means for the occurrence of the higher levels of artistic truth and artistic greatness, both of which are determined by artistic content.

Form as such, the artistic sensuous surface, may be independently enjoyed either by the artist's limitation to it or by limitation of

attention to it. Professor Greene does not recommend such limitation to "pure form" but he maintains that a complete æsthetic experience may be had within it. In relation to the other ends (in general in relation to content) it is only a means. In addition to the discussion of the independent æsthetic status of form (a topic resumed in Part IV), Part II is a systematic and richly illustrated description of generic and special forms, of inter-medial and intra-medial forms, of basic manners of treatment in the special arts and of compositional patterns. It should be noted that despite many statements that the sensitive critic "can delight in sheer æsthetic form or beauty for its own sake" he also insists that "the aspects of art which we are considering *seriatum* have no artistic being in isolation from one another", "that to hypostatise the factors thus isolated . . . to suggest that any ingredient, such as form or content, may be either present or absent without essential artistic gain or loss, is to commit the unpardonable sin in the study of art" (126). The last statement might be thought to comprise both positions, perhaps considering "form" like the "matter of art" æsthetic but of no *artistic* merit, but "essential artistic gain or loss" implies a lesser or greater not a non-artistic experience. And by the critical principles as eventually defined criticisms and artistic appreciation can judge a work of art and even remain on the level of form as such.

Part III, *Artistic Content*, is in large part preparation for the more formal proof, in Part IV, Chapter XXIII, that artistic content may be true or false. The nature of the argument can be briefly indicated. Works of art can be called true or false in some normal sense, "conformable to common usage" for the *generic* criteria of truth apply alike to science and to art, though the *specific* criteria differ. The generic criteria of truth apply only to propositions. Propositions must be expressed in some kind of medium, but it need not be a conceptual medium, that employed by science. Propositions are the "locus" of truth, and should be so defined as to "include every proposition about reality which can be expressed with precision in any medium of communication, including the several artistic media" (427). Accepting, as recognised generic criteria of truth, "consistency" and "correspondence", Professor Greene finds these applicable to art, the first as correctness and felicity in the use of the medium, and as ideational non-contradiction and coherence, the second as "avoidance of empirical discrepancy" and as "satisfaction of all available, relevant, and reliable empirical evidence" (437). The *specific* criteria of art are determined by the difference in media. That the conceptual medium of the sciences is naturally different from the media of the arts, does not make for a generic difference between art and science any more than the specifically different media of the several arts makes for a generic difference among the arts. The media of science are mathematics and scientific prose, its content, "the skeletal structure of the existential spatio-temporal world". Certain aspects of reality "lend themselves far better to

apprehension and interpretation in and through a given medium than any other". "The scientist can apprehend and express the skeletal nature of the phenomenal world with a precision which art cannot rival. Each gets what the other misses, and what one apprehends and expresses in his medium cannot be appehended or expressed with even comparable adequacy in any other medium". Surely the argument is rather an interesting and elaborate analogy than a proof that art and science express truth in the same generic sense.

As aids to critical judgments, the final section, Part IV, *Principles of Criticism,* distinguishes five levels of artistic importance,—formal beauty, artistic quality or perfection, artistic integrity, artistic truth, artistic greatness. Artistic quality is a "function of the success with which artistic form is adapted to artistic intent", and this must be divined by the critic "for the most part from the expressed content of the work of art itself" (390). It is "pure artistic merit" and is "irrespective of the truth and the significance of the expressed artistic content" (374). Artistic integrity is present in a work of art "in proportion as it expresses in an artistically felicitous manner a genuinely coherent interpretation of a given subject matter" (450). (The nature of artistic truth has been explained in the preceding paragraph.) Artistic greatness is a function of depth (which is a function of imaginative penetration and intensity of normative response), and of breadth (which is a function of scope of subject-matter and integrated catholicity of outlook) (467). The interplay of form and content have provided five levels of artistic importance; each level is an end in itself, and so has artistic merit in itself, but as, except for the highest level, that of artistic greatness, each level is also a means to all levels above it, each is less important than that above—it has less *teleological* importance. By this "hierarchical" distribution of relative importance, by the possibility of judgment on each level, and by the level which the work of art reaches, criticism is to be supplied with effective principles of judgment. If a particular critic does not accept all or any of these five levels, he can use the same method of evaluation, even if, accepting only the first level, that of sensuous form, he must treat this as "simultaneously the most 'fundamental' and the most 'important' as these terms have been defined" (481). Moreover, the possibility of independent artistic appreciation of the artistic properties of each level is necessary to the artistic value of the whole, for "each subordinate property can make its appropriate contribution to the work of art as a whole *only* in proportion as it actually possesses an intrinsic artistic value of its own" (482). When the properties of all levels are present in a work of art each can be enjoyed artistically without reference to the further ends it serves, but the highest property—artistic greatness —as an end and not also a means is "pure end, since the ideal nature of art is defined in terms of the achievement of this end" (482). No finality is claimed for this scale of artistic values, but it is

contended that some such results of analysis are " essential to the vitality and integrity of the critical enterprise " (484).

This too summary outline of a somewhat massive argument undertakes to make evident some of the familiar philosophical issues that will need attention in Professor Greene's promised later work, especially those that centre on his interpretation of " organic unity ". But if the reader can in the meantime refrain from philosophical discontents, he will find sufficient compensation in the systematic and skilful exploration of all that enters into works of art in the six major fields, and will have a renewed and more informed admiration of the artist's constructive power over " the mighty world of eye and ear."

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V.—NEW BOOKS.

The Problems of Logic. By ANDREW PAUL USHENKO. London: G. Allen & Unwin, 1941. Pp. 225. 7s. 6d. net.

THIS interesting book raises more problems than are adequately discussed. Professor Ushenko is concerned to maintain that 'an exclusively mathematical treatment cannot give an adequate account of logic' (p. 11). We must recognise that, in addition to formal logic, there is 'the *theory of logic*'. 'This', he says, 'is the medium in which an introduction as well as a discussion of logical forms and of symbolic notation must take place' (p. 14). The distinction between metalogic and logic does not suffice, since, for instance, 'the statement that "$p \supset q$" *is a metalogical designation of the formula* $p \supset q$ cannot be made in metalogic, which has no way of writing the formula, and it cannot be made in logic, which does not use quotation marks' (p. 22). Thus the arbitrary postulational view of logic breaks down. Even if logic be taken as a 'mere game', it 'must preserve distinctions of significance, the distinctions of type between functions and individuals, the discreteness of individuals, the difference between theorems and contingent formulas' (p. 152). The principle of non-contradiction must be *used* even in a system which took "$p \,.\, \sim p$" as a formula, since otherwise there would be no guarantee that the distinctions of significance have been preserved through the successive steps of the calculation. Accordingly, Ushenko urges that the central truths of logic must be known by intuition. He is not unaware of the difficulties in this view; he holds that intuition is self-correcting in that it leads to further intuitions as one proceeds. 'The intuitionalist claims', he says, 'that under *specifiable* conditions his intuition is infallible, although he realises that their actual specification is by no means easy' (p. 31).

In spite of the serious objections that can be brought against this view, Professor Ushenko has raised a problem of importance. I agree with him that deduction is not simply linguistic transformation. I wish that he had seen fit to discuss more fully what he understands by taking logic as a *mere* game, and had dealt more fully with the question what language is. If this question were properly answered perhaps we should understand better the grounds for our acceptance (if we do accept) or for our rejection (if we do reject) of the view that deduction is mere linguistic transformation; we should also be able to dispense with that absurd and misleading word "mere". But Professor Ushenko has not allowed himself space (or is it due to lack of time?) for dealing fully with these problems. He contents himself with arguing that the conventionalists cannot deny the 'objective reference' of language, so they leave it a mystery.

Professor Ushenko touches all too briefly upon the views of Carnap, Russell, Broad, Tarski; he tries to defend the view that propositions must be admitted as well as sentences and facts, but he leaves completely unclear what a "proposition" is. It is to be hoped that in a later volume he will succeed in removing the difficulties that cluster round his use of such terms as *actuality*, *reality*, *fact*. I have the impression that he is still thinking out his position and that later on he may be able to state and to

defend it more satisfactorily. At present his chapters are somewhat disconnected and his treatment is jerky. He discusses types and paradoxes (Chapter II); he thinks paradoxes should be solved individually as they arise rather than by a general theory of types and orders. This method could not afford a general rule for avoiding paradoxes. He has much to say about categories, about the question whether adjectives are names, about the possibility of indeterminate sense-data, and so on. In each case Professor Ushenko's discussion leaves me with the feeling that not enough has been said to make the points clear, but that something more could have been said by Professor Ushenko himself. I hope that in due time he will proceed to say more.

L. SUSAN STEBBING.

The Child's Discovery of Death. By SYLVIA ANTHONY, M.A. With an introduction by Professor J. C. FLUGEL. London: Kegan Paul, Trench, Trubner & Co., 1940. Pp. xvi + 231. 11s. 6d.

THIS book aims at contributing to the establishment of general laws of human development, and in particular at showing how very important is the discovery of death in determining the total emotional and intellectual development of the individual. The study on which it is based was not on a large scale; for that a team of workers would have been required. But it was so well planned and the results combined with previous knowledge with such ingenuity and penetration that the work is not merely suggestive but very convincing, and already gives us a better grasp of the place of death in the souls of children and of grown-ups. To carry it out it was necessary to circumvent the reserves which prevent access to others' minds, falsify what is given, or limit the levels of thought from which it is drawn" (p. 9). With this in mind in place of direct question three methods were used: first, children's spontaneous remarks about death were recorded, mainly by their parents; second, the Story-Completion test; third, the Binet-Simon intelligence test with certain additions. In the story-completion test the child is asked to complete little stories such as: (1) A little boy (little girl) went to school; when playtime came, he didn't play with the others but stayed all alone in a corner. Why? (2) A boy was quarreling with his brother (sister), and their mother came up to them and then . . . what happened? Here resistance is overcome by projection. It may also be overcome by getting the child to adopt the defence of the obsessional neurotic, namely de-emotionalised talk. This is where the intelligence test comes in. "By voice and manner it is suggested to the child that all personal feelings have been set aside" (p. 12).

The subjects for the home records were 11 children from five different families. The subjects for the story-completion and intelligence tests were 117 boys and girls in nearly equal numbers, from ordinary schools, schools for mental defectives and clinics.

In Chapter III the author claims that the story-completion tests in Geneva and in London show that death, whatever it may mean to children, comes readily into their phantasy thought. As to its meaning, she says that "death as sorrowful separation and as the ultimate result of aggression stand out as the main typical connotations of the idea by whatever method we have studied it".

Chapter IV begins with an excellently done inquiry into whether adults think much about death, and continues with a discussion of how sexual

love, including under this such love as Dante's for Beatrice, may make death more bearable. This defence is not available to the child who may be forced to rely on other defences.

In Chapter V the connexion between the development of the idea of death and general intellectual development is considered.

In Chapter VI the very interesting home records begin and are continued in the chapter called "Death, Birth and Hostility". I cannot summarise these nor the penetrating comments on the anxieties connected with these things and on the growth of the idea of impersonal causation. The author suggests that this development depends upon the "dominance of the love aspects of the child's main emotional attitudes and the repression of hate". Death, she points out, doesn't disprove the omnipotence of hate, but it does disprove the omnipotence of love and thus of human will (p. 155). In Chapter VIII it is contended that while "the first step in the path that leads to the sovereignty of conscious Logic and Science is made through the recognition that death is inevitable for others *who are loved*, it is through the association of death with the self that the path is followed to its end". It is interesting to compare Chapters IX and X on "Death and Magic" and "Immortality" with Dicks' chapter in his *Clinical Studies in Psychopathology* on the obsessional neuroses and their connexion with magic and philosophy.

In the last chapter the results of the investigation are summarised and there are useful suggestions as to how to help the child face death.

I cannot here do more towards explaining why this book is so good. It should interest very much those concerned in philosophy and religion. But especially I recommend it to those who, whether by head or heart or both, are interested in children, whether little or grown up.

JOHN WISDOM.

Received also :—

M. J. Bradshaw, *Philosophical Foundations of Faith*, New York, Columbia University Press; London, H. Milford, 1941, pp. x + 254, 16s. 6d.

É. Gilson, *God and Philosophy*, New Haven, Yale University Press; London, H. Milford, 1941, pp. xviii + 147, 12s.

S. Kierkegaard, *Thoughts on Crucial Situations in Human Life: Three Discoveries on Imagined Occasions*, trans. by D. F. Swenson, Minneapolis, Augsburg Publishing House, 1941, pp. 115, $1.50.

D. F. Swenson, *Something about Kierkegaard*, Minneapolis, Augsburg Publishing House, 1941, pp. 173, $2.50.

The Republic of Plato, translated with Introduction and Notes by F. M. Cornford, Oxford University Press, 1941, pp. xxvii + 356, 7s. 6d.

S. W. Baron, ed. by, *Essays on Maimonides*, New York, Columbia University Press, 1941, pp. 316, 25s.

C. de W. Thorpe, *The Æsthetic Theory of Hobbes*, University of Michigan Press; London, H. Milford, 1940, pp. ix + 339, 22s. 6d.

H. Marcuse, *Reason and Revolution: Hegel and the Rise of Social Theory*, Oxford University Press, 1941, pp. xii + 431, 21s. 6d.

F.-L. Mueller, *La Pensée Contemporaine en Italie et l'Influence de Hegel*, Genève, Imprimerie Kundig, 1941, pp. xvii + 345.

F. W. Eggleston, *Search for a Social Philosophy*, Melbourne University Press; London, H. Milford, 1941, pp. 360, 15s.

N. Micklem, *The Theology of Politics*, Oxford University Press, 1941, pp. xvi + 164, 7s. 6d.

A. Gesell, *Wolf Child and Human Child*, London, Methuen & Co., 1941, pp. xv + 95, 6s.

D. W. Harding, *The Impulse to Dominate*, London, G. Allen & Unwin, 1941, pp. 256, 7s. 6d.

R. Lennard, *Democracy: The Threatened Foundations*, Cambridge University Press, 1941, pp. x + 121, 3s. 6d.

K. O. Newman, *Mind, Sex and War*, Oxford, The Pelagos Press, 1941, pp. viii + 82, 3s. 6d.

C. G. Robertson, and W. D. Ross, *John Henry Muirhead, 1855-1940*, London, H. Milford, 1941, pp. 10, 1s. 6d.

H. V. Routh, *The Diffusion of English Culture*, Cambridge University Press, 1941, pp. vi + 134, 3s. 6d.

A. B. White, *Worry in Women*, London, V. Gollancz, 1941, pp. 320, 12s. 6d.

L. L. Woodruff, ed. by, *Development of the Sciences* (Second Series), New Haven, Yale University Press; London, H. Milford, 1941, pp. 336, 18s. 6d.

VI.—PHILOSOPHICAL PERIODICALS.

PHILOSOPHY OF SCIENCE, viii, 2 (April, 1941). Editorial. **W. M. Malisoff.** *On Having a Philosophy.* [Those unable to accept philosophy in any of " the fantastic forms that it presents " can find " a refuge in the attitude of research ".] **P. C. Jones.** *Idealism and Its Relation to Science.* [A summary restatement of subjective idealism. Argues that the truth of " philosophical idealism " is independent of any truths of natural science.] **A. F. Bentley.** *Declassifying Dewey.* [Severe criticism of a paper by E. O. Sisson (in *Phil. Sci.*, July, 1940).] **H. Gomperz.** *The Meanings of " Meaning ".* [An analysis whose interest results largely from its adherence to what Gomperz styles " the principle of terminological conservatism ", *viz.*, the injunction to " keep as close as possible to its traditional meaning " when analysing the variant usages of such a term as *meaning*. (The obvious difficulty, which is not altogether faced in the paper, is of knowing how close " as close as possible " is to be construed as being.) Gomperz distinguishes three main senses of *meaning*, (*a*) as predicated of facts, (*b*) as ascribed to linguistic expressions, (*c*) as equivalent to *implication*. All the " applications " of the term *to mean* are regarded as " different species of a common genus ". Since *to mean* and *to signify* " appear to be used almost indiscriminately " (interchangeably ?) the analysis of the former resolves into an anatomy of *signs*. Subject to certain qualifications, " *s* is said to function as a *sign* for another object or fact *o* " (for a person P) " in so far as P's response to *s* is similar to what would have been P's response to *o* ". Signs are cross-classified by the dichotomies *natural-arbitrary* and *linguistic-non-linguistic*. The meaning of linguistic signs is discussed from the standpoints of pragmatics and semantics, and a distinction is made between the *grammatically meaningful*, the *logically meaningful*, and the *practically meaningful*. Application is made to the principle of verifiability. The discussion is presented as a sample of a method which would " if widely and consistently applied tend considerably to reduce the number and the significance of epistemological problems." But the clarification of the meaning of terms in the way illustrated in the paper " is really the job of philology rather than of philosophy ".] **C. Hartshorne.** *Anthropomorphic Tendencies in Positivism.* [" . . . positivism, whether old or new, is a version of the doctrine of Protagoras, that man is the measure of things. Certain limitations of the human mind are mistaken for characteristics of the universe ". Hartshorne argues (i) that positivists unjustifiably identify the experience of which the human mind is capable in a certain " cosmic epoch " with experience in general, (ii) that such knowledge as is achieved in the sciences presupposes the validity of certain metaphysical propositions (notably " the law that there shall be law, from which the validity of scientific induction as a principle can be deduced "), (iii) that positivism " takes sides " upon a metaphysical question, " the issue of nominalism and realism in one of its more adequate formulations ". It is suggested that " theology is the only radical way to overcome anthropomorphism ". " There ought to be a language deanthropomorphised enough to express a dog's sensations as they are to him, yet

in precise comparison to the experiences of the mind knowing the dog as those are to that mind. Only one such language is conceivable, but it is something more than just a language; the knowledge superior to symbolic thought, realised in the sympathetic intuitions of God."] **E. C. Kemble.** *The Probability Concept.* [Distinguishes between *a priori, inductive* and *theoretical* probability. "In the opinion of the author the long-standing controversy over the meaning of probability is due primarily to the universal but unjustified assumption that the word has a single unique meaning."] **A. Kardiner.** *Psychoanalysis and Psychology. A comparison of Methods and Objectives.* [Describes, in a general way, the aims and methods of Freudian psychoanalysis.] **W. J. Norton, Jr.** *Towards a Value Theory of Mind.* [Argues that "no understanding of mind can be arrived at unless two things at least be shown: (1) that mind presupposes objective values; *i.e.,* rests upon them for its own intelligibility, and (2) that in itself mind implies an active attitude towards the attainment of values by seeking their actual expression."] Reviews. Announcements. Technical Scientific Section.

VII.—NOTES.

ACHILLES AND THE TORTOISE.

In the January number of Mind (Vol. 50, 1941) there is an article by J. O. Wisdom on Zeno's paradox of Achilles and the tortoise. It contains an interesting discussion of several interesting philosophical questions. But to a mathematician some of the discussion seems irrelevant so far as Zeno's paradox is concerned.

To the mathematician, Zeno's "paradox" is not a paradox at all, but a simple and clearly understood fallacy in mathematical reasoning. The problem is a purely mathematical one, involving no question of physical measurements, but only logical relations between pure mathematical quantities. The fact that there is a lower limit to the size of the space intervals that can be measured does not justify a positivist attempt to invalidate the logic of the Calculus. In order to avoid irrelevant complications, let us forget Achilles and the tortoise, and substitute for them two material points, A and T, moving in the same direction on the same straight line. Let A have a velocity of one mile per hour and T a velocity of $\frac{1}{2}$ mile per hour. At a given instant let T be one mile in advance of A. The statement of the paradox then becomes:

While A is covering the one mile, the original distance between T and A, T moves $\frac{1}{2}$ mile further along and the distance between them is $\frac{1}{2}$ mile; while A covers this $\frac{1}{2}$ mile, T moves $\frac{1}{4}$ mile and the distance between them is $\frac{1}{4}$ mile, etc. We thus obtain an infinite series 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, —. The terms of this series express the successive distances between A and T. Since no term of this series can ever become zero, no matter how long the process continues, the distance between A and T can never become zero, *i.e.*, A can never overtake T.

To the mathematician this reasoning is entirely clear and sound, *up to the word "never"*, but the *never* is a plain mathematical *non sequitur*. Zeno's reasoning seems plausible to some, because he uses skilfully the familiar psychological scheme of misleading by so wording the problem as to draw attention away from one element of the situation.

Let us state the same thought in slightly different words. During the hour required for A to move the one mile, T moves $\frac{1}{2}$ mile further on. Therefore at the end of the first hour the distance between them is $\frac{1}{2}$ mile. During the next $\frac{1}{2}$ hour which A requires to move this $\frac{1}{2}$ mile, T moves $\frac{1}{4}$ mile, and therefore at the end of the first $1\frac{1}{2}$ hours the distance between them is $\frac{1}{4}$ mile. During the $\frac{1}{4}$ hour required for A to move this $\frac{1}{4}$ mile, T moves $\frac{1}{8}$ mile, and therefore at the end of $1\frac{3}{4}$ hours the distance between them is $\frac{1}{8}$ mile, etc. We thus have an infinite series of space intervals, the terms of which express the different distances between A and T at different times. No terms of the space series can ever become zero, therefore A can never overtake T. Here the mathematical fallacy stands out more clearly. Success in misleading the unwary depends much on the way one words the problem.

The simple mathematical conclusion, of course, is not that A can *never* overtake T, but that A cannot cover the sum of the series of space intervals separating the two in less time than the sum of the series of time intervals required for A to cover the corresponding space intervals. In other words, A cannot cover the two miles necessary to overtake T in less than the two hours required for him to move two miles.

Evidently, our "Paradox" disappears as soon as we take account of *all* of the elements of the situation and reason logically about them.

We have in our situation *two* interrelated infinite series, one of space intervals and the other of time intervals, each of which converges to a limit. Zeno states his puzzle shrewdly to draw attention away from the time series. His psychological trick reminds one of the familiar conundrum: "A blind beggar had a brother. That beggar's brother died. What relation was the beggar to the man that died?" This is a perfectly straightforward logical question of relationship. But at first thought many persons feel sure that the answer is, He was his brother, which is wrong.

Zeno's "paradox", as a problem of logic, is no paradox; only a more or less skilfully concealed mathematical fallacy. But the discussion of it through the centuries since his time is an interesting subject for psychological study.

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DOES A CONTRADICTION ENTAIL EVERY PROPOSITION?

This question was asked in a paper of mine, and Mr. Karl R. Popper (MIND, Vol. 49, 1940, p. 408) answers it in the affirmative. I was satisfied at first, but on further thought am again doubtful. The argument is: (1) p entails (p or q); (2) not-p and (p or q) entail q; hence (3) p and not-p entail q. Now it seems to me that the interesting question, if we think a system containing even one contradiction worth discussion, is whether it can contain only one. The argument considers the situation if the system contains a particular pair of contradictory propositions p and not-p. But then in (2) we infer q from not-p and (p or q) by denying the possibility that p and not-p can both be true. This assumes that the system does *not* contain the contradiction (p. not-p) assumed in (3). If we assume p and not-p, then not-p and (p or q) are together consistent with (p and not-p); thus q does not follow. My point is that if we choose to accept both p and not-p, and wish to consider whether they entail any other proposition, we must not also consider them inconsistent. To do so assumes a second contradiction (r. not-r), where r is (p. not-p), and the question at issue is whether there is a second.

I should agree, of course, that if contradictory propositions appear in a system, other contradictions can usually be deduced, and that they can certainly be deduced if we accept and deny the same contradiction; and this would be enough for most of Mr. Popper's argument. On the other hand, there is a kernel of truth in the Hegelian contention, that science proceeds by discovering contradictions and resolving them; but this can be dealt with better by constructing a theory of probability including ordinary logic as its extreme case than by rejecting the latter.

The question seems to be relevant to the justification of the use of mathematics in science and in the theory of probability. Carnap and others have given proofs that pure mathematics is free from contradiction, but for epistemological reasons similar to those advanced by Russell in *An Inquiry into Meaning and Truth* I am indisposed to accept the whole of Carnap's system. Now Carnap is so drastic as to take $\sim p \,.\, \supset \,.\, p \supset q$ as his first primitive sentence. What happens if we read $\supset$ as "entails"? The formalism stands, and Carnap's discovery of a proposition not entailed in his system, including the unmodified law of contradiction, is a proof of consistency. But PS1 cannot be interpreted in this sense in probability theory, since it would have to be read as "if p is false, the probability of q on data p is certainty". Thus the verification of any prediction q could never support a true hypothesis $\sim p$ against a false one p, and scientific method would break down completely. The situation is then that PS1 (in this sense) can be assumed without inconsistency but is not part of the scientific use of mathematics. But if a system is consistent when an additional axiom is added it must be consistent without it; hence Carnap's result does lead to the conclusion that ordinary mathematics can be used in science without inconsistency.

HAROLD JEFFREYS.

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